Volume 35 JANUARY, 1940 Number 1

The Journal of Abnormal and Social Psychology

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PUBLISHED BY
THE AMERICAN PSYCHOLOGICAL ASSOCIATION

EDITORIAL OFFICE: EMERSON HALL, CAMBRIDGE, MASS.

PUBLICATION OFFICES: ALBANY, N. Y., AND
THE OHIO STATE UNIVERSITY, COLUMBUS, OHIO

Entered as second-class matter, June 18, 1923, at the Post Office at Albany, New York

SYMPOSIUM: PSYCHOANALYSIS AS SEEN BY ANALYZED PSYCHOLOGISTS

Editor's Note

Since its founding this Journal has been one of the chief mediums for the impartial discussion of psychoanalysis. In its very first issue, in April, 1906, J. J. Putnam described Freud's new method of treating hysteria through "Psycho-Analysis." In 1910 the editor, Morton Prince, explained that owing to the increasing significance of the Freudian movement it would hereafter be the policy of the Journal to discuss frankly disturbances of the sexual life, adding, a bit apologetically, that for a proper understanding of psychoanalysis "it is unavoidable that the physiological and psychological details of this side of the human organism should be laid bare." (There is no record that the Journal's readers protested this breach of decorum in its pages—not even in 1910.)

Freud's recent death gives renewed impetus to attempts to estimate the permanent significance of his work, both for therapy and for psychological theory. Although it is unlikely that final agreement will be achieved for a long time to come, it is important to encourage continuous free discussion and argument concerning the many issues involved. For this reason the Journal welcomes any serious attempt to evaluate psychoanalysis as a branch of scientific psychology.

The following symposium was inaugurated on the assumption that "analyzed" experimental psychologists should be authoritative judges of psychoanalysis, since they have had training in scientific procedure as well as first-hand acquaintance with psychoanalysis in the role of analysand. The number of these especially qualified judges, though limited, now seems sufficiently large to make possible a symposium of their views.

There follow four studies expressing in varying ways the evaluations of psychoanalysis reached by four psychologists who have completed their analyses. The symposium is not written from the analyst's point of view—one exception being where the analyst has commented on an autobiographical discussion. For the most part the authors of these articles address their remarks not to psychoanalysts, but to psychological colleagues, with the purpose of communicating to them some of their first-hand experiences and their considered—if not final—judgment of the value of psychoanalysis. The symposium will be continued in the next number of the Journal.

WAS THIS ANALYSIS A SUCCESS?

BY EDWIN G. BORING
Harvard University

IT seems strange that a psychologist after his psychoanalysis should not have a message for his colleagues, when there has been so much questioning of psychoanalysis by the orthodox psychologists; and yet it is true that four years after my analysis I still cannot assess with assurance the significance of the experience in my life. Since, nevertheless, my hesitation is a datum in itself, I welcome this opportunity to make it in specific detail a matter of record. Apparently psychology is not yet in a position to validate or invalidate psychoanalysis experimentally—with selected groups and carefully chosen controls. Hence we are reduced to the collection of case histories; and critical autobiographical histories by sophisticated, scientifically minded persons ought to be worth more than the enthusiasm of naïve persons about an event which has helped them. With this conviction I join the present symposium, realizing that no considerable part of the total picture of psychoanalysis can ever reside in the private experience of a single person.

The reason for my undertaking psychoanalysis was primarily therapeutic, a need arising out of the basic tenets of my professional creed. My ambition has always been strong, my entire life formed about a drive for scientific productivity. Such success as has been my lot seems to me to have depended more upon persistence than genius, for my ethic has been that a successful psychologist works at his job eighty hours a week and fifty weeks a year. Though I think now that this drive has grown out of my need for approbation ("affiliation," "love"),[1] I did not before my analysis recognize that origin. Viewed from the distance of the present, the 1920's seem to have been for me a fairly effective decade, and my *A History of Experimental Psychology,* a successful book as psychologists' books go, appeared in 1929. After

[1] Since the psychoanalysts' *love* seems never quite to achieve the broad meaning that they intend for it, it is less ambiguous to speak of *need for affiliation,* after H. A. Murray, *Explorations in Personality,* 1938, *passim;* see esp. 320–2, 719–21, 743.

that I got into another job of writing, *The Physical Dimensions of Consciousness* (not nearly so good a book as the other), which I finished in the spring of 1932. Later in that year I found myself in an emotional crisis, a crisis so accurately timed that I wonder sometimes if in some vague way I did not sense its coming, if I may not even have driven the second book through to completion that it might be done before the crisis should occur.

When it came, the crisis seemed to leave me sterile, troubled with autistic thinking and all the interferences with attention that arise from emotional disturbance. About the sterility or its cause one cannot, of course, be sure. Presumably every ambitious academic person passes periodically—perhaps even annually—through depression, periods during which he is beset by the conviction that he is making no headway. *Ex post facto* I see that the 1930's have been less effective for me than the 1920's (and this in spite of the analysis!); yet the cause is not certain. The decade of age 33–43 is likely to be more productive than the decade 43–53, so Lehman's curves seem to prove.[2] It is perhaps enough to say that in 1932–33 I was depressed, that I seemed to achieve little, that the thought of my professional accomplishment's diminishing rather than increasing in the future seemed utterly intolerable to me. So evident was my emotional disturbance that many of my associates urged analysis upon me, and into psychoanalysis I went, therefore, as a last resource. My difficulty was psychological, and I was a psychologist. It seemed that there must be some way whereby I could regain full control of my attention, and, since I knew of nothing else, psychoanalysis was worth a try. In public I saved my face by the rationalization that the psychoanalysis of an experimental psychologist might result in some important insight into the relation between the two fields.

The first analyst I saw repelled me. He seemed too mercenary. We had not talked half an hour before he explained that, were I to miss any conferences, I should have to pay just the same. I know, of course, the reason for that rule; yet he seemed to be thinking less about my health than about his own pocketbook.

Then I met Dr. Hanns Sachs, one-time Director of *Psycho-*

[2] H. C. Lehman, The creative years in science and literature, *Sci. Mo.*, 1936, 43, 151–162; The creative years: "best books," *ibid.*, 1937, 45, 65–75; see also W. R. Miles, in E. V. Cowdry, *Problems of ageing*, 1939, 562–564.

analytisches Institut at Berlin, a pupil and friend of Freud's. I liked him at once. He reminded me of Titchener, whose image has always dominated my professional life. I still feel a warm friendship for him, uncertain as I am of the effects of his analysis. He is wise in general knowledge, in history and literature, although the rigors of positivistic science are not for him. Ready as he was to learn about American psychology, he never could have liked it. We arranged for the analysis in December, 1933, but Dr. Sachs was too busy to take me on before the next September. So desperate was I then that I did not see how I could wait three-quarters of a year; yet I had to. I mention this point to show that my initial attitude was favorable to a successful analysis.

From September 1934 to June 1935 I had 168 analytic sessions, five every week. I missed none by inadvertence or on short notice, although by planning for professional engagements long in advance we skipped a few. I had what I think was my share of emotion. I wept. I threw things. And once, right after a session, I found myself quite unmoved by a lecture from a traffic policeman, so much calmer was that usually enraging performance than the analysis.

The expense seemed to me to play the role that was expected of it. The analysand is supposed to make a sacrifice in order to obtain the analysis; this sacrifice secures his serious cooperation. I knew how many seconds cost one cent, and often, as I lay on the couch awaiting the next free association, I found the new idea would be an image of the pennies scurrying by as costly seconds were lost without any progress in my therapy.

So I liked my analyst, I suffered, and I felt impoverished—all favorable auspices for a successful analysis.

The analysis was not didactic. Most people, I think, do not realize how little of conventional psychoanalytical theory and terminology there needs to be in an analysis. My analyst had no belief in word magic. He did not care what terms were used so long as we worked ourselves clear as to the meanings. He was, moreover, dealing solely and particularly with ME and not with the human mind in general. It was I, not he, who wanted to make the generalizations.

Now we come to the difficulties.

All through the analysis I felt the inadequacy of the psycho-

analytic philosophy, a philosophy which seemed to me to be a loose and indeterminate mixture of voluntarism and determinism. This failure of the discipline to take a firm stand on so basic a matter troubled me greatly—and still does. I recall that toward the close of the analysis the analyst reiterated, "Where there's a will, there's a way," implying that, if you do not do what you can do, then you do not want to do it. That philosophy roused my ire, for it was offered as a motivation, not as a mere tautology. After all, psychoanalytic therapy is founded on a belief in the inadequacy of voluntarism and in the meaninglessness of the concept of will.

I do not believe that I can be said to have got the intimacy and efficiency of personal relation with my analyst that is called *transfer*. I liked him, and sometimes I was angry with him. He was a very important person in my life that year, but I doubt that he was much more important than a few others, like the president of my university, who can determine, I should suppose, depression or elation for every departmental chairman, since it is the president who decides whether the chairman shall prevail or fail, whether he shall be wise or foolish. There are those who will say that this fact proves my analysis to have been unsuccessful. The point is that I did not know then, nor do I know now, whether my analysis was "successful"; and my analyst himself has never pronounced his judgment. I gathered that he thought it would have been successful if I thought it was; but I needed him to tell me what I should think.

It seems certain to me that we fell far short of the recovery of old memories which are made so much of in the literature. I think it is true that we came to no assurance about any event of my childhood that was not already in my normal repertoire of reminiscence. Though we tried to work on only a few dreams, none of them came to that self-consistent closure which is supposed, I understand, to indicate a correct dream analysis. It would seem that I have no important hidden past, or that it is irrelevant to this psychosis, or that the analysis failed.[3]

There is one objective fact about my analysis which I must add to complete the record. It may have been crucial, though I

[3] Perhaps I was too old at 48 for psychoanalysis, but opinions differ on the adaptability of the ageing to analysis; see G. V. Hamilton, in E. V. Cowdry, *Problems of ageing*, 1939, 465.

think it was not. The situation of 1932 that threw me into analysis was suddenly altered in the spring of 1935 toward the close of my sessions. Thus the analysis, which started for the purpose of clarifying one situation, ended by needing to clarify another. Since it was I myself more than the particular situation that wanted clarifying, I do not see how this event could have changed the analysis from success to failure. Psychoanalysis is supposed to be something more than the mere practical adjustment of individuals to particular situations, needing to be repeated whenever life presents a new unpleasant surprise.

We come now to the main issue. Did my analysis help me to understand myself better? Did it give me a new insight into my motivation, and was there, with this insight, any new appreciation of reality that should permit me to dismiss my foolish emotions for what they were and return my attention to psychology, where I wanted it to be? Let us consider the question of insight first, and of therapy second.

As to insight. I was certainly confused before the analysis. Believing as I did that the most important thing in my life was my professional ambition, I went to analysis to have it unentangled so that it could proceed. But I also believed then that my need for personal relations ("affiliation") and for approbation was fundamental. It seemed plain to me that the damaging of the latter had injured the former, though I was fairly astonished that there should be anything in my own constitution that could thwart my ambition. Nevertheless, I entered analysis without a clear belief as to the causal relation between personal needs and professional ambition. I entered it, more than that, with some sense of guilt about pressing for the right of those personal needs to exist. What the analysis did was to sanction these needs. If it did not discover them, at least it reassured me in respect of them. For a year or two after the analysis I felt a little suspicious, fearing that analysis had proved only what I should have liked it to prove; but I confess now that I have no misgivings about the result, that I am assured that the analysis is right, and that it is a mild relief not to feel guilty about this sector of my egoism.

Now as to therapy. It is true that I hoped for a personality change. I did not like what I had. There lay, however, a mystery in psychotherapeutics. I could understand how free association

could reveal the hidden parts of the mind, but I never could see how their revelation could alter the personality. Over this riddle I puzzled from September to March, being nevertheless fairly patient, for I am an experienced subject in experiments and know better than as subject to take on responsibility for an experiment. By April, however, I was beginning to worry. There ought, I thought, to be some revelation if the analysis were to be complete by June—and I "had" to complete it by June. I had not the money to continue, nor did I have the time. Too many colleagues were being inconvenienced by the necessary adjustments to the schedule of my appointments. In May I began to be alarmed, but there was no help coming from the analyst. Then in June I found that he was going abroad on the 22nd and that my last session would have to be on the 21st.

By the 15th I was asking desperately what to do about my personality, which, for all the emotional upheavals of the past few months, seemed to me still no different from the outmoded bundle of incompatibilities that I had had to get along with for so long. To my question there was only a partial answer. It was on the 20th, I think, that we agreed on the fact that the analysis had reached a point of diminishing returns and that it would be unprofitable to continue in the fall, and then the analyst cautioned me as to the impossibility of knowing whether the analysis had been successful until some time afterward, perhaps a month. I ought, he said, to get clearer about it in time. That to me was an unsettling anticlimax. I had eagerly awaited a light from heaven, at the very least to be changed from Saul to Paul; and all that happened was that the analysis petered out in an uneventful session on June 21st and my analyst went abroad!

The after-effects were somewhat worse than the analysis. I was distraught. I had tried a last resource, and it had failed. Yet, unwilling to accept so bitter a conclusion, I found myself seizing on the analyst's casual statement that I ought to wait a month. I waited anxiously, hoping for a new personality by July 21st. None came. Finally I sought out my psychologist-friends who believe in psychoanalysis, and we sat in conference discussing this sad immutability of my personality—on August 21st, as I suddenly realized. Their advice was patience, the less haste the more speed; wait at least until December 21st, they urged. So I waited, through all of one sabbatical semester, a

semester during which I had planned to write on the other volume of my *History of Experimental Psychology*. That precious sabbatical, filled as it was with the emotional reverberations of the analysis, was an utter loss. Not only was I unable to write; I could not even read effectively. And finally I ceased to expect a miracle. I was sad and distraught, knowing that I had wasted one of the four precious sabbaticals that are due me during my life.

Now, four years after the close of the analysis, I find myself quite uncertain as to whether it has made any important change in me. Too many other things have happened for me to say why I am as I am. I have not yet regained the productivity that I so confidently expected in 1932, but for its loss, of course, I can find an alibi in academic administrative responsibilities. I am somewhat calmer about this thwarting of ambition; but as to whether that calm is the result of the analysis or of my advancing age, with only fourteen years now left before my retirement, who shall say?[4] True, the analysis has had this advantage: having been analyzed, I can no longer feel that I have left any available stone unturned. But that would be a rather small return for so large an undertaking. Perhaps I am a little more aggressive. Perhaps I speak up more readily with the confidence that what I have to say is important. Perhaps less often do I meet contradiction with the conviction that the other man is presumably right. But I am not at all a calm or well-poised person. My "need for affiliation" is still dominant. Cruelty and arrogance—whether in Germany or at Harvard—have an effect upon me so disturbing that the newspapers can throw me out of work for an evening, and a simple local act of aggression, not even directed at me, can stop creative work for half a day. There is so much about this personality of mine that would be better if different, so much that analysis might have done and did not!

[4] Cf. W. R. Miles, in Cowdry, *op. cit.*, 555–562; also G. V. Hamilton, in *ibid.*, 464–479.

WAS THIS ANALYSIS A SUCCESS?—COMMENT

BY HANNS SACHS
Boston, Massachusetts

EVERY analyst of sufficient experience has observed the therapeutic effect of psychoanalysis, but it remains still an open question in how many cases anything approaching a full cure can be achieved and how the result compares with the necessary sacrifice of time, exertion, and money. Any attempt to clarify these problems deserves an analyst's most intense cooperation. I shall, therefore, try to describe what I consider to be the fundamentals of the problem and point out its difficulties. The discussion of Professor Boring's paper will serve as an excellent approach to the general viewpoint.

I see no reason to contradict Professor Boring's statement which terms his analysis, with some minor exceptions, unsuccessful. I cannot raise an objection when he declares it as doubtful that the favorable changes have been the effect of his analysis or rather of extraneous circumstances; but I want to add the warning that these "doubtfuls" will be present with almost every therapy—except of an acute illness—and will be absolutely unavoidable in psychotherapy where the psychical adaptations and the modifications of reality work hand in hand in a most insidious manner.

The causation of this partly absent, partly doubtful success may, of course, lie in the inefficiency of the analyst, who made some mistake or overlooked some important aspect. Of this, I cannot myself be the judge. Anyway, there are some objective factors which might furnish a sufficient explanation. The analysand in this case, although suffering in various ways, had no neurotic symptom. It was clearly a case of character-analysis which has to be considered from a different angle than a strictly "therapeutic" analysis of a clearly defined—and diagnosed—neurosis. In such a case it makes all the difference if the character has retained some of the infantile flexibility or if it has been cast a long time ago in a fully developed and strictly preserved form. With a personality like the one before us, where,

for example, professional pursuits are not a spurious interest that could be given up easily for the sake of other aims and where the claims of family-life have a deep and lasting influence on the character formation, a start for a new way of living, of a totally different emotional life, offers not much chance. This fact does not mean that psychoanalysis or even character analysis has to abdicate unconditionally in face of such reality-factors as are evident or could be ascertained before the analysis. Age, for instance, is a highly important factor, but it cannot be justly evaluated on the sole basis of the number of years. I want to emphasize that all these facts, such as age, professional life, family ties, money situation, etc., have to be taken into consideration, not merely in regard to their objective and present importance, but still more in relation to the question of how strong their hold on the personality of the analysand is, how far they form an intrinsic, inseparable part of it, and what role they played in the evolution of his character from early childhood on (sublimation). In technical terms this means the investigation of the super-ego and of its infantile origin.

When the analyst thinks he has gathered sufficient material to decide these questions, then—but only then—he has to face the two fundamental problems of a character analysis. First, is the power which analysis has at its disposition and which consists (for the sake of brevity I use technical terms again) of the libido that has not been transformed permanently into super-ego-functions—is this power equal to the task of producing a fundamental change of character? Secondly, is it advisable in the best interest of the analysand to attempt such a task, even if one cannot be sufficiently sure of the success? These questions are quite different with a neurosis or with a case where the sublimation cannot be maintained any longer and, showing already the fissures of an impending neurotic breakdown, has therefore to be considered as equal to a manifest neurosis. One does not hesitate to wreck a slum-tenement which endangers the life and health of its inhabitants, but one feels differently in dealing with a dignified and respectable house which has given comfort and shelter for a long time, even if it causes to its inhabitants a good deal of inconvenience.

In one point my opinion about the therapeutic value of this special analysis differs from that pronounced by Professor Boring.

It is about the "one objective fact" of which he says: "It may have been crucial, but I think it was not." This fact amounted, as I see it, to a potential trauma; that is, I have some good reason to assume that this fact, without the analysis, would have produced the danger of a breakdown in the sense mentioned above, probably tending towards depression. What might have happened cannot, of course, at least for an outsider, constitute a convincing proof as compared with what *has* happened. But this circumstance, as already mentioned, is one of the constant difficulties of psychotherapy. I may add that even in this point I do not want to claim the beneficial influence as a consequence of psychoanalytic technique in its strict sense. A life-long, very intimate friend, to whom the analysand could have confided in many repeated and unrestrained outpours all his woes and worries, might have done him the same service. But it was not accidental that such a friend did not exist; and I put it down in favor of psychoanalysis—and not as a small thing, either—that it was able to provide on short notice a stopgap for the missing friend when such a stopgap was most urgently needed.

In taking a more general view of our problem it has to be kept in mind that the healing of mental and organic illness has proceeded on different lines. In the beginning they were identical; the primitive mind conceived every pain and sickness as the working of magic, as the influence of evil spirits or of malevolent witchcraft. Consequently all therapy was magical too. With the progress of civilization and reality-adaptation the scientific view, regarding organic illness as the result of disturbing processes in the organism, was soon—though far from completely—evolved. Mental healing remained for a much longer time on the magical level; the witch-expelling exorcism was still in universal use at the beginning of the eighteenth century. A scientific attitude, making psychogenic causes responsible, was, even among the most progressive minds, not existent till the middle of the nineteenth century and seems generally even today far less acceptable than in the matter of organic illness. All scientific knowledge in psychotherapy is therefore comparatively new; it has not only to fight against resistances which elsewhere do not exist, but it carries still not a few traces of its origin from a belief in magic. The expurgation of these traces is one of the major claims of psychoanalysis; it means that not only the hope

for working miracles, but even the aim at such effects has to be abandoned.

I have mentioned already that from a purely therapeutical—not so much from a psychological—point of view the situation is quite often rendered neither perspicuous nor unequivocal, because of the constant intermingling and interaction of psychic facts and modifications of reality. I will quote a case in illustration; yet, even in selecting as clear-cut a case of therapy as I can find in my experience, I feel a pang caused by the necessary over-simplification. The patient, who had been an addict first to morphia, then to cocaine, had, after a pause which had been the result of an unfinished analysis (conducted by another analyst), taken to drinking. The alcoholism had lasted several years in the most severe form when he entered the analysis with me. In the meantime other methods of psychotherapy together with several stays in nursing homes, closed institutions, etc., had been tried without the least success. During the third year of his analysis he became free from the addiction for several months. Then came a full relapse. The analysis lasted approximately four years and resulted in full abstinence. A few months after the end of the analysis he had a relapse which lasted two weeks. Since then, for nearly seven years he has been absolutely free from drug addiction in any form. I consider this as a signal success of psychoanalytic therapy, and I feel proud and happy when I think of it.

But it is not for this reason that I quote it here. There is a hitch in the story. Just after the end of the analysis a total change in the surroundings and circumstances of the analysand took place. This change, it is true, was by no means on the easy side. On the contrary, he needed all his determination and resourcefulness to see it through. It is also true that I never heard of a severe case of alcoholism, at the verge of delirium tremens, cured by a change in the reality-situation. But who can answer the question—except purely theoretically—whether or not just this sort of emergency at just this particular moment was necessary to set his feet on the road to full recovery?

Another obstacle to the correct appraisal of the therapeutic value of psychoanalysis is the absence of any yardstick or general rule concerning the following facts:

1. The seriousness and extent of the neurosis.
2. The strength of the forces opposing the therapy (primary and secondary gain, reality-situation, traumatic experiences, etc.).
3. The extent and the duration of the therapeutic result.

It would be much easier, if we could find at least a strict interrelation between these three factors, but even this is not the case. As to the first, patients which are in fact near-psychotic may impress not only their acquaintances but even the psychotherapeutist for a long time as fairly normal, whereas others who are called perfectly crazy by themselves or by their friends suffer only from a passing neurotic spell.

This is not the place to discuss the second point adequately; the deeper factors—e.g., the amount of masochistic satisfaction which the patient acquires by his symptoms—would need a detailed exposition. Instead of it I will mention one of a more superficial kind. A patient who wants to be cured of a neurosis becomes aware that he cannot set himself free without giving up an important gratification, such as a pervert phantasy, masturbation or certain habits to which he is addicted, and he reacts accordingly.

The difficulty of forming a right estimate of the extent of therapeutic success has been referred to in this discussion. I shall add here that the members of the patient's family behave sometimes very much like the father of the dumb girl in Molière's *Médecin malgrè lui;* when his daughter has been cured of her mutism and breaks out in an endless streak of wild talk, he implores the doctor who cured her to make her tongue-tied again. Some patients behave to the therapeutist in a manner like that of a criminal to the lawyer who has got him off: they deny that they ever were seriously involved, and they want to forget all about it.

I cannot end this discussion without giving my personal opinion of the problem as precisely as I am able to formulate it. Psychoanalysis is—as Freud has emphasized repeatedly—far from being the ideal therapy, since it is neither certain, nor quick, nor easy. That admission, however, does not alter the fact that psychoanalysis is getting nearer to the roots of the evil than is any other therapy; instead of taking care of the surface, psycho-

analysis works on causation. It is, therefore, not only the most thorough therapy but in a good many cases the only efficient and durable one available at present. Since it has given, moreover, an opportunity for the fullest investigation, progress in other methods would not have been possible and probably for some time will not be possible without the help, instruction and instigation which psychoanalysis gives to them. For the psychoanalyst the therapy is the indispensable instrument for any advance in his science and almost the only source of material that furnishes possibilities of gaining new insight. In the matter of the proof and verification of psychoanalytic theories, the psychotherapeutic proceeding gives opportunities to check up on single instances, but I should never consider it possible that psychotherapy in isolation from the rest of psychoanalytic investigation and experience should be used for this purpose. The proof—I repeat that I am voicing my personal views—lies in the combined force of all the experiences, tests, and explorations which can be made over the whole range of psychic life, pathological as well as cultural, and brought together after each of them separately has been demonstrated as fully as its special nature permits.

PSYCHOANALYTIC PHENOMENA

BY CARNEY LANDIS
Psychiatric Institute, Columbia University

Personal Statement

My conscious motivation for entering into a psychoanalysis was threefold: (1) I desired more intimate personal knowledge of the phenomena discussed in psychoanalytic literature in order that I might more adequately interpret and present the results of an extended research on psychosexual development; (2) I had taught abnormal psychology some 15 years and had never been satisfied with my presentation of psychoanalytic material; and (3) I hoped that certain personal problems of an affective nature might be alleviated by the psychoanalytic process. The analysis was financed by a Fellowship granted to me by the General Education Board, Rockefeller Foundation.

My analyst was a close adherent of Freud and to the best of my knowledge did not deviate in the slightest from the orthodox Freudian method and theory. He had been actively practicing psychoanalysis in Europe and America for over 25 years.

I had 221 analytic hours over an elapsed period of 15 months. My classification of the general subject matter discussed during 221 hours is as follows:

Dreams and associations with dream material	30 per cent
Recounting of immediate environmental emotional difficulties	20 per cent
Recounting of childhood and adolescent memories	15 per cent
Unimportant associations (resistance)	15 per cent
Personal attitudes, their meaning and history	10 per cent
Recounting of physical ailments, or mental symptoms	4 per cent
Discussion of current events	4 per cent
Discussion by the analyst	2 per cent

Whether or not I was "completely" psychoanalyzed depends on the standard of completion one chooses. As will be apparent, I did experience most of the phenomena which many psychoanalysts have described as typifying the analysis. There were distinct personal gains and losses in the process. I was not

"converted" to the point of unquestioning belief or to emotional acceptance of a doctrine or system. In answer to questions my analyst sometimes replied, "When your analysis is complete, you will understand that without further explanation." I never achieved such complete understanding of all points of doubt or question. If completion of an analysis means conversion—I was not analyzed. If completion means a degree of understanding and satisfaction of the immediate conscious motivation which led to the analysis, then I was completely analyzed. Freud has said that an analysis is ended when the patient no longer comes to see the analyst.

I suppose that most professional analysts would say I was not completely analyzed, whereas certain of my non-analytic psychological acquaintances have told me that I had gone entirely too far into analysis. I myself feel that I "wouldn't take a million for the experience, or give a nickel for any more of it."

The psychoanalysts' reports of phenomena which occur during analysis constitute a most impressive literature. Even larger is the literature bearing on their interpretation of the phenomena and the ways in which these data are pertinent to the central theoretical construction outlined by Freud. Too frequently the non-analytic psychologist has been repelled by the interpretation and theory and has ignored the phenomena educed by the analysis which are of unique psychological interest.

It is my purpose neither to praise, condemn, nor convert. Rather I wish to point out certain phenomena which occurred during the analysis and whenever possible to explain them in the more usual terms of general psychology.

ANXIETY

After about ten or twelve hours of analysis (one hour a day, five days a week) I found that I was possessed by a thoroughgoing anxiety which pervaded all my thinking and interpersonal relations. At first there was no tendency for this apprehension to attach itself specifically to any event, person, or class of relationships. Rather it colored my existence in the same way that colored glasses tint the visual field. I was never particularly puzzled by this anxiety, although I was bothered by its persistence. From the very start I could identify the feeling tone or affective coloring and could place it in my life history. My

father believed literally in the scriptural injunction, "Spare the rod and spoil the child." As a child I had been catechized concerning my misdeeds and failures to follow parental orders. All too frequently at the end of such a report I had been told that I would be switched that evening or the next morning. In the meantime I was filled with apprehension waiting for the thrashing which, when promised, I always received.

The anxiety feeling in analysis was to me identical with my childhood apprehension and was set up in a very similar fashion. The analyst had said, "Tell me about yourself, be perfectly truthful, and talk about anything you wish." I followed directions. The only difference between this confessional with the analyst and the accounts I had given thirty years previously to my father was that the analyst said nothing during my rambling accounts and did not mention a thrashing. Hollingworth has called this process emotional redintegration. The Freudians call it cathexsis. Actually the close similarity of situation reinstated an identity of emotional mood.

At other times during the months of analysis there was a recurrence of this anxiety for longer or shorter periods. But always it was set up when I told of some event or relationship in which I had participated and for which I had a sense of self-reproach concerning my conduct. My father would have said, "Your conscience does and should bother you." The analyst commented on a tyrannical Super-Ego.

An interesting development in this anxiety experience in later stages of the analysis was its tendency to dissociate from the original circumstances which set it up and which I myself was satisfied had set it up. The apprehension entered into all kinds of judgments and behavior. Because it took constant conscious effort to discount the effect, at times an interesting mental state of semi-dissociation intervened. I would watch myself behaving or talking in an anxious fashion, aware that the responses were emotionally overcharged, yet not seeming to care sufficiently to correct them.

IRRITABILITY

At times during the analysis I experienced prolonged periods of irritation and a general bellicose attitude. This irritation usually followed a period in the analysis during which I had

talked at length concerning a problem I was attempting to solve or decide, or when decision or solution so baffled me that I continued puzzled and undecided. It also appeared when I had stripped myself mentally of some familiar explanation and came to the realization that my belief had been irrational in spite of the fact that I had previously thought that belief to be rationally determined. Like the anxiety, this irritability was generalized and colored the entire mental life. My explanation (to myself) of the irritation was that I had divested myself of my usual and ordinary basis or reference points for decision or self-explanation. When any event or dilemma demanding a decision occurred, I had no method of reassuring myself of the soundness of my action. I was deprived of all usual guiding precepts. There was a distinct "conversion" nature in this affective experience. I thought of the decisions as headaches and of the autobiographical accounts as "stripping" myself—the hurt that followed was the hurt of a headache or of the peeling skin after sunburn.

TRANSFER

Since so much has been written and said about transfer and its importance for the success of the analysis, I tried to observe it closely. I found it vastly overrated. One lies on a couch and talks. One tells one's hopes, fears, indiscretions, sins, ideas, fancies, and history. The only previous occasion for indulgence in such a complete talk-about-myself was in childhood. Then I would tell either my father or mother how it all happened, ought to happen, or might have happened. Sometimes my parents had commented. Usually I had prattled on. Unless the story concerned some misdeed about which I was being quizzed, the parental attitude had been one of mild interest. The situation with the analyst was a complete reversion to childhood. I was telling all. Occasionally the analyst asked further detail, but usually he sat quietly at the head of the couch while I talked and wondered whether he was listening, much as I had wondered whether my parents were listening. Again I'd ask for advice, for consolation, or for sympathy as I had as a child. In all it was a reinstatement of a childhood—an early childhood attitude. Of this I was quite conscious and referred to the analyst in my thinking as Papa.

Still another element entered into this situation. My attitude throughout the analysis was a simple-minded one of following orders wihout quibbling. In return I felt I could ask any question I pleased of the analyst. Frankness should be returned by frankness. On the basis of his years of analytic experience he should give answers and advice, just as my father or mother had done when I was a boy. Often the analyst's answer to a question was, "When we are farther along, you'll understand." My father had said, "You'll know when you're older."

So the relationship with the analyst was consciously one of son-father. But that I loved the analyst—no. I respected him—yes; again just as I had my father.

RESISTANCE

After having told and retold my autobiography and immediate troubles some three or four times I found my conversational powers waning. The analyst did not encourage the discussion of current events, psychological research problems, and other topics which usually constitute the greater part of my everyday thinking and conversation. In fact, when I did spend time on such subjects, he would sigh and say, "Ah, resistance again." So I asked what I should talk about and was told to allow my thoughts to wander and to talk of anything that came into my mind. But it was apparent that the analyst was pleased by certain selected topics in this process of association. If I talked of my infancy, childhood, adolescence, or other early family relationships, no comments about resistance were heard. So I sought to allow my mind to go back to childhood and to talk on and on about boyhood life and fancies. Occasionally this was easy. A flood of memories would take me, and for several days I would seem to please with my reports. Then I'd run dry, or the immediate cares and preoccupation with everyday existence would blot out all memories of childhood. I could not get back to such material. I'd block in thinking and talk in half sentences. There would be long silences, after which I would finally talk of present preoccupations, and the analyst would remark at the end of the hour, "Resistance again; I hope tomorrow's hour will be better."

It did happen occasionally when I had recounted a childhood experience that the analyst would point out an interpretation or

meaning which occurred to him with which I disagreed. This disagreement usually grew out of the fact that the analyst had an urban European background while mine was that of a small American middle western village. I am certain that he no more understood the cultural pattern and its attendant emotional tone than I would have understood his account of his boyhood in Europe if he had recounted it to me. This fact constituted a problem to me. I must attempt to report so he would understand and so I would not create misapprehension and misunderstanding. The next hour I would be more precise in reporting, and he would sigh and say, "You hate me today for what I said yesterday and so are showing resistance. Don't be so precise. Let your thoughts go as they will." Of course he was right. The purpose of the analysis was to help me understand something about myself, not for me to educate him about boyhood in Ohio.

After more than 200 hours of analysis the resistance became more and more marked. I had told and retold childhood events. My dreams led me into no new lines of free association. New insightful flashes into my own motives and thinking became more and more rare. So my analyst suggested I break off the analysis for some months, seek other lines of activity, and then resume later. This I did. But when I came back after eight months, it was almost immediately apparent that the resistance to the flow of free associations in desired channels was as great or greater than when I had previously discontinued the analysis. After five or ten hours of trying to get started I told the analyst I didn't believe that it was worth the effort, and as a result finally discontinued the analysis.

The resistance plus the recurrent phases of anxiety and irritation made the entire analysis a most unpleasant and unhappy experience for me. Week after week of trying to talk with nothing to say became a veritable nightmare.

UNCONSCIOUS MEMORIES

That many forgotten events, emotional and unemotional, were recalled during the analysis was most apparent but not strange. If one revisits a scene, once familiar but long forgotten, the associated detail of forgotten memory which comes into the foreground of thought is well known. Or if one sets himself the

task of mentally reconstructing the past in as great detail as possible, it can be done with surprising completeness. This is what happened. I strove to recall childhood memories and emotional events. Each day's memories led on to others. As a consequence I was able to exhibit to myself many occurrences which, I believe, were of importance in the formation of my own adult attitudes. I comprehended the why of many attitudes marking my everyday behavior. In this connection I see no need to speak of unconscious motivation or of unconscious dynamics. It was a redintegrative process in which adult responses carried with them an affect identical with that of many childhood experiences of a similar character.

In this process of recall I found, for the most part, that my attitudes sprang from training—formal and informal—which was not particularly difficult to recall. One group of pervasive attitudes did work out as lineal associations stemming from a strict boyhood religious training. The full significance of this connection was longer in becoming apparent. Evidently I had repudiated the entire mass of memories in my effort to free myself from nightmares concerning unforgivable sin, hell fire, and eternal damnation.

DREAM ANALYSIS

Since dream analysis forms a very important panel of Freudian analytic procedure and since I found dream recounting one of the most productive ways of starting desirable free associations, I did devote a large share of the total analytic time to dream material. For me this part of the analysis worked out in a rather unexpected way. I recounted dreams at length and in great detail. The immediate associations were easy to make and frequently revealing of wishful and phantasy thinking. Most of the time the more manifest symbolism was easily arrived at. In a few instances the dream as a whole was quickly realized to be representative of a wishful attitude. But usually, after recounting the apparent or manifest dream and a certain amount of associated ideas, I blocked. No more material came to mind, and many of the suggestions of an interpretative nature which the analyst made I rejected. This rejection was psychologically interesting. The analyst would say, "What about this association?"

or, "It seems to me that such and such was said by the dream." So far as I was concerned, he was either right or wrong, immediately and completely. I could either say, "Yes, that's right; never thought of it that way, but you're perfectly right," or, I would say (or think), "Papa, you're wrong on that one. I never thought or felt that way in my life. Maybe you did or maybe the books say I should, but I didn't." And these judgments of correctness or incorrectness were sudden and to the best of my knowledge were not altered later.

In many of my dreams railway trains or locomotives played a role. This recurrent element puzzled me. I've always been interested in railroads and locomotives, but what did they mean in dreams? I finally worked out an explanation which the analyst said was on an immediate manifest level, but he never gave me a better one. During the time of the analysis I was living near a street-car line and at night the passing cars frequently awakened me. When I was a boy, my home was very near a railroad, and the roar of trains often awakened me as a child. As a child, furthermore, railroads meant to me going away to cities, an escape from the parental discipline and farmwork which I hated. I think the rumble of the street car reinstated the memory of the roar of the trains, and the roar of the trains in childhood phantasy was associated with the idea of running away from trouble. Freud says the locomotive in the dream is a death symbol. For me this was not true. Running away did not mean dying or death; it meant escape and independence, a shaking off of inescapable annoyance and trouble. The analysis itself was an annoyance and trouble. I did not enjoy reinstatement of memories of my childhood which were not particularly happy, and I hated the mental blocking—the resistance—which stopped association. I wanted to escape these annoyances, and the recurrent dream element offered an escape idea. But my interpretation was not along orthodox lines and hence rejected—or so I thought at the time.

For several reasons dreams were excellent taking-off-places for free association. They were fantastic. They were often childish. By their nature disconnected and illogical, their recounting set up disconnected and illogical thoughts. Since they were made up of a potpourri of elements, there were many possible lines of departure. The analyst could select any of the elements and

say, "Doesn't this mean that?" If I accepted, fine; if not, nothing lost. But, best of all, the dream attitude of easy reverie is exactly that most conducive to free association of a wandering sort which may bring out forgotten memories and associations which furnish the clue to the style of reaction in many similar events.

NEUROSIS

After I had spent forty or fifty hours in analysis, the analyst remarked in his explanation of a dream, "That's one aspect of your neurosis." I asked, "Have I a neurosis?" "Why certainly." That set off a long soliloquy on normality and neurosis. I asked, "What is normality?" He replied, "I don't know. I never deal with normal people." I asked, "But suppose a really normal person came to you." He interrupted, "Even though he were normal at the beginning of the analysis, the analytic procedure would create a neurosis." Then I realized he was absolutely right on that point. I have already described the anxiety, apprehension, irritation, resistance, and mental befuddlement that the process of enforced free association set up, and these are all cardinal symptoms of neurosis.

Today when we have National Committees set up to study the problems of neurosis; when we see $1000 prizes given because a scientist has created a "neurosis" in a rat; when a determined effort is being made to create neuroses in sheep and pigs in order that they may be understood in humans, it seems a bit pathetic that the leaders of the analytic group who have done so much to clarify the nature of neurotic phenomena should not step forward and plainly say, "We know how to set up a neurosis, and we know how to alleviate and frequently cure that neurosis. True, we don't exactly know the physiology or the neuropathology of the process, but we damn well know how to concoct it, just as a baker knows how to make bread though he knows nothing of the process of fermentation or of biological chemistry."

THE PSYCHOLOGY OF THE ANALYST

The literature is full of psychoanalytic descriptions of patients. The psychology of the analyst goes almost unreported and unstudied. This is a gross oversight, for in the analytic situation there are two individuals, one quite as important as the other;

and yet the reporter (the analyst) with altogether too much modesty fails to report in any adequate fashion how he got that way, how he got there, and why he says what he says.

Consider the external setting of the analyst's life. He spends practically all of his waking days, eight or more hours a day, six or seven days a week, sitting in a chair at the head of a couch listening to rambling free associations of a neurotic patient. He makes notes of pertinent points and occasionally intersperses comments, interpretations, guiding hints or advice. During all these hours he must listen (or pretend to listen) to repetitious, egotistical, egocentric chatter about the innermost personal life of his patient. He must bear the reproaches of the patient. He must continually listen to the frustrations, to the thwartings, to the blighted hopes, to the unfulfilled wishes, to the major and minor sins, to the loves and hates, to all that is infantile and childish. He never or but rarely hears the positive or fine accomplishments in the life of his patient; and when he does hear of them, he cannot but remember the height, the depth, and the smell of the other side of that same existence. He must listen or daydream his own phantasies.

Usually the analyst is a physician who has been attracted into psychiatry with little or no knowledge of formal or academic psychology or of philosophy. He knows little and cares less about studies of learning, maturation, and emotional expression. Education and educational methods are to him a closed book. Unless he is a most unusual man, he has had but little time for literature and the arts. His training in science and the scientific method is that of the skilled technician, most of whose training has been of the follow-directions variety. He has been told over and again that psychology and philosophy are the bunk, and that all there is to education and educational method a high school girl of slightly less than average intelligence can grasp in two years.

Feeling the need of some positive therapeutic procedure with which to approach the pressing problems of the mental patients, or hearing that analysis is a wide-open field offering rich financial returns, he enlists for an analysis and such training courses as may be required to qualify him for analytic society membership. In these training courses he finds he must believe or else. . . .

And after such preparation he sits and listens. The marvel is that analytic reporting is as adequate as it is. The reporter, having

no scientific psychological vocabulary, invents a terminology or uses that which Freud invented.[1] Since he knows but little of experiment or experimental control, he cannot correctly evaluate his results. He knows next to nothing of the years of study on instinct, so he adopts a century-old outmoded theory of instinct and thinks as he has been taught to think.

After several years in the practice of psychoanalysis he acquires skill and art in his methodology. He finds by trial and error that certain remarks can be made and desirable responses obtained, that other remarks and advice must be strictly avoided or his patient will quit or become too violent. He becomes more cautious and adopts a more and more passive attitude. He avoids experimentation. He perfects a procedure which brings about a changed attitude in a fair number of his patients. When he tries to tell how it works or why it works, he may follow either of two policies. He may paraphrase an account given by Freud (or one of Freud's close disciples) with his own case experience fitted in, plus several case histories. Or he may branch out to do some explaining on his own. Since he has only a psychoanalytic jargon with which to report his findings, they usually sound pretty awful to the non-analytic scientist.

And yet he really has something to say. He tries to describe how a neurosis comes about and how it may be relieved. Scientists, psychologists, and ordinary physicians are baffled and annoyed by his terminology and ideology. "Why can't the man report so we may know what he is talking about?" He cannot, since he has no adequate knowledge of scientific reporting, no comparable technical vocabulary, and but the haziest ideas of scientific logic. That is the tragedy of the analyst.

PSYCHOLOGY AND PSYCHOANALYSIS

I believe that the procedure used therapeutically by the analysts brings about a wide variety of psychological manifestations of interest and importance to the general and experimental psychologist. It is certainly possible to set up emotional attitudes,

[1] No reflection on the invention of a terminology is intended. The point is that new facts and relationships require new names. It is usual in science, however, to develop a new terminology out of older vocabularies, so that cognate meaning and understanding are possible. Definition of new terms in a precise fashion is scientifically desirable. The analysts do not use their new terminology either precisely or in a fashion which lends itself to ready comparison or comprehension in terms of other psychological knowledge.

moods, and prevailing affects, and to control their appearance and nature within wide limits. Past events and phantasies can be recalled and reinstated with a large share of their original emotional vividness. Very interesting dissociation phenomena can be produced. Feelings of unreality can be set up and studied. Distortions in the temporal component of consciousness can be produced and studied, since they occur in a diverse fashion. All this and more, in addition to the oft-reported genesis of attitudes, repressions, transference, etc., are available.

Personally I believe that the phenomena are as much a result of method and procedure as of any basic personality structure revealed by the method. Another method, a changed procedure—and the dogmatically described dynamics of the unconscious which form personality structure according to Freudian theory would be seen from an entirely different angle. Suppose all the stress were placed on adolescence and the struggle of the child to become independent of his family. The dynamics would be entirely different. Suppose, in place of free association, controlled association held to the realm of the acquisition of reading and arithmetic in school rather than that directed to phantasy or infantile sexuality were used. Suppose the analyst would soft-pedal all sex reports and emphasize fear or anger. Certainly the personality picture reported would be most different. One hasn't the faintest idea how these things might turn out, but one is certain that the results would be of psychological interest and importance.

It seems to me that the general experimental psychologist might avail himself of this technique and general procedure to bring out interesting material. The experimentalist need not interest himself in therapy or healing but could actually go about his problem experimentally. The experienced analyst could furnish many rules and guiding percepts of a practical sort. Murray pointed out several years ago a plan for training psychologists.[2] One wishes that his plan might be utilized.

[2] Murray, H. A. Psychology and the University. *Arch. Neurol. Psychiat.*, 1935, 34, 803.

THE POSITION OF PSYCHOANALYSIS IN THE SCIENCE OF PSYCHOLOGY *

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INTRODUCTION

As an academic psychologist who has recently completed a training analysis I meet people everywhere who want to "pump" me about my experiences in and my opinions of psychoanalysis. Not only in social intercourse but in the discussion periods following occasional addresses or technical papers, the same series of questions almost invariably comes up. A few of these queries are pertinent and sophisticated, but the vast majority are confused and naive. I regret to say that those asked by my colleagues who are professional psychologists are as often in the second as in the first category. There are psychological reasons for this state of affairs, some of which will be given in a later section of this paper. My chief purpose in writing it, however, is not to tread on toes but to help make the discussion of psychoanalysis by psychologists more intelligent.

In a controversial field the questions of both the bias and competence of the author are bound to arise. I had studied the psychoanalytic literature and had had a considerable bias in favor of psychoanalysis for some years previous to my own analysis. I have expressed this bias in some previous papers (5; 6; 7). A brief quotation from one of these (7) will indicate my theoretical position before analysis.

> The academic psychologist who has really studied psychoanalytical theory (and he is a very rare individual), is apt to be overwhelmed by the systematic complexity of the theory and by the importance of the problems with which psychoanalysts are concerned. Be he Titchenerian Structuralist, Behaviorist, or

* From the Psychological Laboratory of the University of Kansas and the Menninger Clinic. Much of this paper is condensed from the author's forthcoming book, *An introduction to psychodynamics*, to be published by the McGraw-Hill Book Company, New York, 1940. The author is indebted to Dr. Franz Alexander for permission to use his analysis of the sources of error in psychological observation and for many suggestions for improving the text. He is also indebted to Dr. Karl Menninger for a careful, critical reading of the original text and for valuable suggestions towards its improvement.

Gestalt-theorist, he must in honesty admit that his own particular school neither answers nor even poses questions of such wide systematic implication nor of such vital interest. If he is a teacher he must be further impressed with the fact that psychoanalysis is concerned with just those problems which his students expect him to answer and for which he has no answer, at least from the standpoint of his own theoretical persuasion. Psychoanalytical theory deals with *psychological problems*, it deals with *nearly all* psychological problems, and the problems basic to it are *vital*.

The academic psychologist, however, has some advantages which many psychoanalysts lack. He has been trained in the logic of science and in the experimental method. As a logician and an experimentalist, he feels, and rightly so, that many aspects of psychoanalytical theory are in need of conceptual clarification and of a somewhat more rigorous type of proof. He has been trained to accept as scientific those statements concerning experience to which competent observers give universal assent. And although he does not consider himself competent to gather the data which psychoanalysts use in making their theories, he does consider himself competent to criticize the manner in which these theories are built. He further considers himself right in demanding from psychoanalysis that type of experimental criticism which is essential to the healthy growth of any science. To put the present status of both psychoanalysis and academic psychology briefly, the psychoanalyst is equipped with an extensive knowledge of the most vital psychological facts, but he is not equipped to deal with these in critical experiments. Most academic psychologists continue to use the experimental method on facts which are all too often banal or simply *curiosae*.

The outcome of this state of affairs is most unfortunate. The psychoanalysts are inclined to consider the academic psychologists curious fellows in no way concerned with those real problems which they as psychotherapists have to meet. The psychologists, on the other hand, are inclined to look on the psychoanalysts as "mystics" or "cultists." Nothing is gained by either side and the resulting disdain and suspicion are mutual.

Since my analysis, although I now accept some of the contentions I previously criticized and reject some I previously accepted, by and large the judgment remains unchanged. The process of being analyzed, however, has given me insight into why I hold such opinions. And this fact may give my present paper more authority. I by no means hold to the often-expressed dictum that only the analyzed have any right even to entertain an opinion about psychoanalysis. I do, however, feel my own opinions are much better founded since going through analysis than before.

By psychoanalysis, I mean that portion of psychology which deals with the sources and distribution of the psychic energy underlying the integrated behaviors of the whole organism. As such it embraces three different fields of endeavor which, although interrelated, must be evaluated separately. These three different fields are:

(a) A method of *psychological observation* in which free association and dream analysis and study of the transference of early acquired libidinal and aggressive attitudes towards the analyst are used to uncover the unconscious.

(b) A systematized *set of theoretical constructs* which are used to order the psychological data found by this method.

(c) A method of *psychotherapy* in which these techniques are used in a special sense to change the structure of the human personality.

A great deal of confusion arises in that critics, failing to consider these fields separately, draw implications from one field to another when such conclusions do not follow. Thus one often hears that since the *therapy* is long and expensive, the *method* is worthless and the *constructs* methodologically inadequate. Or it is alleged that the *therapy* is very successful but owes its success to suggestion, so that both the *method* and the *consructs* are not worthy of consideration.

Such statements are of course illogical, but I hear them *ad nauseam*. In this paper, consequently, I shall try to evaluate method, system of constructs, and therapy separately with regard to the adequacy of each in its own field. I shall also have something to say about the methodological shortcomings of each and inquire how far these may be avoided. Psychoanalysis represents a vast amount of research, and its literature is tremendous.[1] I shall make no pretense, therefore, of elucidating the details of the theory, nor will I give scholarly documentation. The few appended references provide easily available sources where the topics in question are treated more or less extensively.

A METHOD OF PSYCHOLOGICAL OBSERVATION

No matter what one thinks of psychoanalysis as a systematic set of concepts or as a psychotherapy, one is "forced" to admit that it is a method of psychological observation of great importance. Some time before the word *psychoanalysis* was invented, the fact of unconscious motivation of both psychological and physiological processes had been established. William James called this discovery the most important made by psychology during his lifetime. It is well known how the combined efforts

[1] Cf. the imposing *Index Psychoanalyticus* compiled by Rickman (19).

of Bernheim's school and Charcot's (plus the at times acrid debates between them) established the fact that hypnosis is created by suggestion (8; 17). They then showed that hypnotic suggestion could influence both psychic and physiological processes, so that the symptoms of conversion hysteria could be artificially created in hypnosis. From here it was easy to prove that hysteria itself is a sort of "self-hypnosis" in which both physical and psychological symptoms are produced without the conscious cognizance of the patient. There is no point at this time in quibbling about the methodological status of the concept "unconscious." We must accept the fact that *some behavior originates from motives of which the subject is not aware in his normal waking state, but which may be discovered in hypnosis by a specially trained observer.*

If the science of psychology is to cover all the phenomena of human behavior, methods must be devised for the observation of these unconscious mechanisms. What is needed is a sort of "psychological microscope" by which the psychologist can observe in the subject those processes that he cannot observe with the unaided eye and ear. Now it may be that sometime we shall have a "physiological microscope" so that the bio-chemistry and bio-physics of the nervous system may be observed directly. That time, apparently far away at the end of the nineteenth century, seems just as far away today. The "apparatus" for such a psychological microscope cannot even now be a "brass instrument" but must be a condition of observation.

As we have already seen, the discovery of the laws of hypnosis leads to the possibility of using hypnosis as such an instrument.[2] Freud began to play with this new instrument with certain neurotics. When, after a period of about five years' experimentation (1890–1895), he discovered that the method of free association was superior to that of hypnosis for the purpose of observing unconscious motivation, he began to use the former exclusively. It would take us too far afield to go into Freud's reasons for replacing hypnosis and suggestion by free association (9; 10). The chief of these, however, is that in free association not only

[2] The discovery of law and instrument is always closely interrelated. The viewpoint that empirical observation aided by instruments leads inductively to law is seriously to be questioned. Knowledge of laws more often creates the possibility of making new instruments than do existing instruments create the possibility of discovering new laws. Cf. Brown (4).

the observer but also the observed becomes gradually cognizant of the unconscious factors involved and so the study of total life histories is greatly facilitated.[3] There have been now some forty years of extensive experimentation with free association by Freud and his followers. I think we must accept the fact that *most human behavior originates from motives of which the subject is not aware in his normal waking state.* These motives, however, may sometimes be discovered by psychoanalysis. Psychologists, furthermore, are themselves human beings and similarly influenced by unconscious motives, a fact which led Freud to the belief that psychologists themselves should be analyzed.

Dr. Franz Alexander (1) has given us a very keen analysis of the sources of error in unaided psychological observation and has pointed out how psychoanalysis as a method overcomes or at least minimizes these. According to him there are four sources of error in every communication regarding motives between two individuals, such as the psychologist and his subject.

The first of these is the danger that the subject will limit his communication to what he wants to tell the experimenter. He may wish for reasons which are quite conscious (pride, prestige, guilt) to place himself in a different light. He may, in other words, lie to the experimenter. Academic teachers know how frequent such behavior is in students suspected of cheating in examinations. Psychoanalysis as a method minimizes this source of error, practiced as it is chiefly either on ill individuals who desire to get well, or on candidates who themselves wish to become psychoanalysts. Both categories of individuals will tend to be honest, particularly as confidence in the analyst is established. Resistance (i.e., the persistence of the wish not to face the unconscious basis of our motives) and transference (i.e., the personal emotional relationship which arises between analyst and analysand) enhance this source of error, however, and help make psychoanalysis a long and laborious process. It is often urged that psychology has more objective methods ("lie-detectors") to determine whether individuals are telling the truth. Although it is true that such devices may detect deception, in themselves they cannot create honesty. The use of methods of measuring

[3] That free association has greater therapeutic value than hypnosis and that not all individuals may be hypnotized are other important reasons.

the physiological concomitants of emotional states during analysis is not impossible and perhaps in the future will be regularly undertaken.

The second source of error (which we have already dealt with in some detail) lies in the fact that the subject does not himself know all his motives. In the process of psychoanalysis (the free association method and dream analysis) he gradually becomes cognizant of them. The extent to which this awareness occurs depends, of course, largely on the length and adequacy of the analysis and on the structure of the subject's personality. Physical microscopy itself depends on the power of the lenses and the nature of the specimen under observation. That analysis is a long process and probably never completed cannot be held against it as a scientific method. It is the best we have.

The third source of error lies in both the experimenter and the subject. They may not know each other or not "speak the same language." This source of error is largely overcome by the daily interviews between the experimenter and the subject, taking place over a considerable period of time. Since basic similarities and differences in the personalities of both parties do play a role, however, the choice of the best suited analyst is a matter of extreme importance.

The fourth source of error resides in the experimenter. The psychologist like any mortal is subject to unconscious motivation. If his own unconscious has been explored, however, he may compensate for this effect. He may learn his own "personality difficulties" and psychological "blind spots." Communication is a bipolar relationship. The subject may not know or want to tell; the experimenter may not know himself, or wish to hear. The demand that psychoanalysis be practised only by thoroughly analyzed individuals minimizes this source of error.

Seen from this viewpoint, then, psychoanalysis has no parallel in the science of psychology as a method of studying human motivation. Psychology has other methods which are better suited to other problems, such as perception and learning. For the study of the dynamic sources of total human behaviors, however, analysis is unique and indispensable. It would be an exaggeration to state that the sources of error of unaided communication are eliminated in psychoanalysis. They are only so far minimized as our present state of knowledge permits. Further

technical refinements may be expected in the field of "psychological microscopy" just as they have occurred in the field of physical microscopy.

We may summarize by stating: *granted the role of unconscious motivational processes in determining human behavior, psychoanalysis is a uniquely "successful" way of studying them.* This conclusion stands independent of the methodological value of psychoanalysis as a set of constructs or its value as a psychotherapy.

A SYSTEMATIC SET OF THEORETICAL CONSTRUCTS

A vast array of psychological facts has been gathered through observing humans by the method of psychoanalysis. I think it is no exaggeration to say that more scientific information concerning the springs of human action has been accumulated through forty years of psychoanalysis than through the centuries of unrefined psychological observation. These facts have become ordered and systematized through the aid of certain theoretical constructs.

Psychology has come to methodological maturity. The naive belief that science is solely a product of empirical observation unaided by either theory or guiding principles has been given up even by the modern behaviorists. Science consists of a set of propositions about nature to which competent observers assent. That the propositions are observable under certain controlled situations is the reason why such assent is obtained. No sensible person thinks of debating with physicists unless he knows physics. The same state of affairs should be, but is unfortunately not, true in psychology. Everyone claims a "right to an opinion," even without knowledge of the facts. I must state dogmatically that certain facts have been established by the psychoanalytic method which are as undebatable as the facts of thermodynamics or gravitation, albeit of course not so mathematically deducible or easily demonstrable. It is not my purpose in this paper to touch on these facts except incidentally as they are integrated with the theory.

The facts of a science, once established, become systematized through theoretical constructs. The history of psychoanalysis has been methodologically sound in that fact and theory have been organized hand-in-glove fashion from the time of Freud's earliest

writings. We accept the necessity for some sort of coordinating definitions and theoretical constructs. My purpose in this section is to inquire into the adequacy of those adopted by the psychoanalysts. In a single paper the whole theoretical structure of psychoanalysis may not be examined. Elsewhere (6) I have tried to show how the theory is related to the various theoretical positions in academic psychology. Consequently here we shall examine methodologically only certain of the outstanding theoretical propositions.

The theory of the basic urges (1; 12; 13; 15). The theory that the basic sources of human psychic energy are to be found in the libidinal and aggressive instincts is the *starting-point* but by no means the *cornerstone* of psychoanalytic theory. Inasmuch as Freud was forced to accept something to start with, as a late nineteenth century biologist he accepted what all nineteenth century biologists believed in, namely the instincts for racial and self-preservation. These he called the libidinal and the ego instincts. The realization that hate was as great a force as love came much later (1920). In the world as we know it today this statement needs no particular proof. Hate and aggressiveness are not rare and pathological but frequent and "normal." The question of *where* both the libidinal and the aggressive urges originate is, however, a different one. Freud accepted them as instinctual (i.e., innate, teleological, and occurring without foresight into the end), but of course subject to environmental modification.

Modern methodological research led by Dunlap in psychology and Bernard in sociology has raised severe doubts as to the adequacy of the concept of instinct. Some of their criticisms are undoubtely applicable to certain writings by psychoanalysts. But psychoanalysis as a systematic psychology stands quite independent of the theory of instincts. Although Freud himself retained his faith in the instinct theory, some of his followers no longer accept it. Whether the libidinal and aggressive urges are instinctual or early acquired is of vast importance for the implications of psychoanalysis for sociology (it is claimed, for instance, that "war is inevitable" or that "socialism is impossible" because of the death instinct), but of practically none for the development of a systematic psychology. No matter where they come from, the facts of love and hate are ever with us.

I wish but can scarcely believe that these lines will prevent sociologists and psychologists from dismissing psychoanalysis with a sneer because it is "based" on a theory of "instincts."

The theory of the structure of the personality (12; 14; 1). The naïve belief that human beings are controlled by unitary conscious souls or egos is today held only by fundamentalist theologians and by the completely uneducated laity. We have already seen that much of the energy for human behavior lies in the unconscious. Besides the unconscious, psychoanalysts posit the conscious and the foreconscious. These represent the topographical divisions of the human psyche, i.e., the locations in which the changes in dynamic energy take place. The earlier Freudian theory supposed that behavior was the resultant of conflicts between the conscious and unconscious elements of the psyche. After 1902 Freud (14), realizing the necessity for positing directive forces within the psyche, developed the theory of the ego, super-ego, and id. Thus beside the topographical problem (i.e., the problem of the location of conflict) there arises the dynamic problem (i.e., the problem of the nature of the conflicting forces).

In addition to the topographic and dynamic problems every behavior also presents an economic problem. Freud states that every conflict is resolved by the dynamic redistribution of psychic energy in the most economical fashion possible considering the total situation. Thus a psychosis is economical when it is the only alternative to suicide, or a conversion hysteria economical when it is the only alternative to continual psychic conflict. The Freudian theory thus supposes that all behavior results from the disequilibration of the existing distribution of *psychic energy*. This disturbance gives rise to conflict. A new equilibrium is established through the *dynamic forces* (id, ego, super-ego) working out the conflict in the *topographical segments* (conscious, foreconscious, unconscious) of the mind in an *economic* fashion.

There is today little doubt that any theory of the mind must allow for its structuredness. The only question which arises is whether the Freudian theory is the "best" which can be made. Of all the various theoretical constructs of psychoanalysis those concerned with the structure of the person seem to me to be the least adequate methodologically. In general, psychoanalysis has done more than any psychological school to overcome the theory

of animistic souls characteristic of pre-scientific psychology, but in the concepts of the ego, super-ego and id much animism still remains.

Although these concepts are realized to be of an auxiliary nature by most analysts, occasionally one comes across statements by psychoanalysts which describe a given behavior as the result of a personal quarrel between a nasty little homunculus called the id, a moral little one called the super-ego, and a selfish little one called the ego. These creatures seem to live in not very definitely circumscribed areas of an ill-defined place called the psyche. The conceptions of many analysts of conscious and unconscious are also much too rigid. The idea of the economy of psychic processes is not at the present time subject to logical or experimental proof. I believe that the conceptions of the topological psychologists are methodologically more adequate to describe the now well-established fact of the multiple-structured person and its relations to the environment (6). In fairness it must be admitted that the research of the psychoanalysts much more than that of the topologists has established the fact in question.

The theory of psychosexual genesis (13; 15; 20). All modern theories of personality agree that personality has its genesis in the way in which organisms of specific biological constitution overcome or fail to overcome environmental barriers. The chief arguments are concerned with the nature of the basic urges and with the nature of environmental frustrations. The psychoanalysts suppose that the personality has its genesis through the frustration of the basic libidinal and aggressive urges. To develop this theory requires the demonstration of infantile sexuality and hostility. That Freud conceived of sexuality in a very broad sense is finally being realized. In this sense I think we can say that infantile sexuality is an established datum of psychology. That very young babies (certainly under one year of age) show marked libidinal and aggressive attachments (to parents and siblings) is now scientifically established. That the vast majority of neonates live for a period of time in a family is not debatable. That sexual "perversion" is normal in children and is a frequent phenomenon in adults is known to all psychiatrists. With these facts in hand the Freudian postulate of infantile sexuality becomes an indispensable one for genetic psychology.

The status of the theory of psychosexual development (as opposed to the postulate of infantile sexuality) is but little more open to question. We have already seen how personality development is a dialectical process in which established personality organizations become disequilibrated through the frustration of emerging urges, so that a new organization ensues. In choosing the frustrations concomitant with weaning, toilet training, and infantile masturbation on which to build the theory, Freud has certainly chosen most if not all the major frustrations of childhood in our culture. Undoubtedly other frustrations concomitant with other new adjustments such as walking and talking are of importance in child development. Freud has touched on these only incidentally. Undoubtedly also, as the anthropologists have shown, if one changes the nature of the culture (i.e., of the frustrations), personality differences ensue. But the fact remains that as long as human beings are conceived through copulation, are born of the womb, are nursed as mammals, are forced to eliminate urine and faeces under socially controlled conditions, and are sexually differentiated, Freud's "Three Contributions to the Theory of Sex" remains a basic text in psychology.[4]

We may summarize this section by stating: *granted the necessity for theoretical conceptions in systematic science, those of psychoanalysis are the most adequate we have in accounting for the sources and distribution of energy in the integrated behavior of the organism as a whole.* They need methodological and logical refinement and demonstration by a more direct experimental approach. As concepts they do not have the same degree of scientific adequacy as does psychoanalysis as a method of psychological observation. Again, it must be borne in mind that their validity is equally independent of the efficacy of psychoanalysis as a method of psychotherapy.

PSYCHOANALYSIS AS PSYCHOTHERAPY

Academic psychologists are medical laymen and, like all medical laymen, inclined naively to accept the "myth" of the infallibility of the physician. They are, however, students of the mind and inclined not to accept the "myth" of the infallibility of the psychotherapist. They are hence more inclined to emphasize the failures than the successes of psychoanalysis as a therapy.

[4] For the thought of this sentence I am grateful to the anthropologist, Dr. Weston La Barre.

Psychoanalysis as a psychotherapy is based on Freud's finding that the unconscious motives made conscious by psychoanalysis as a method of observation were then subject to conscious intelligent control. Its efficacy as a therapy depends, then, on the skillful use of the method and on the use of the conscious intelligence of the subject. Since even in the longest psychoanalysis the whole unconsciousness is not brought to light and since no intelligence is perfect, immediate limitations are placed on psychoanalysis as a therapy.

Psychoanalysis as a therapy is not, as it has often been called, a "mental surgery." Certain pathologic organs—an inflamed appendix, tonsils, and the like—may be excised without seriously upsetting the integration of an organism in surgery. Surgery is chiefly concerned with structural pathologies. The development of certain other pathologic processes may be prevented by immunization in the blood stream or sterilization of the environment. Through the field of medicine, however, there are functional pathologies which are not subject to excision, on the one hand, and have already developed, on the other—such as diabetes, hypertension, tuberculosis. For these medicine has no cure in the sense of single specific treatments such as operations or immunizations. They must be treated by arresting the pathologic process and then controlling its future course. The success of the treatment depends on the stage of development of the pathogenic process and its severity.

Most mental diseases are functional pathologies. The idea that a phobia or a mania which has its etiology in a long psychic development may be excised by "mental surgery" is absurd. What may be done is to arrest the particular psychological development which makes it inevitable. There are various psychotherapies which specifically attempt such a check. Of these psychoanalysis is probably the most ambitious because it aims at a fairly complete examination of the unconscious. It attempts, in other words, a causal rather than a mere symptomatic cure. But even if psychoanalysis is the most ambitious of the psychotherapies it does not pretend to remove "complexes" in the way that surgery does tumors, and it cannot assure complete immunity from further "complexes" in the way that vaccination assures immunity from smallpox. What it usually does is to

teach the patient the sources of his complexes so that he can understand them and live with them. Occasionally, however, the complexes themselves disappear in the process of becoming conscious.

Although psychoanalysts are more and more accepting research cases, in general only those with pecuniary reserves can afford to do so. The psychoanalyst is a highly trained medical specialist whose practice is limited to around seven patients at any period. The psychoanalysis of the individual (for reasons we gave in the first section) is a long process. Consequently, except for research cases, only the well-to-do may be psychoanalyzed. In general, youth, if not a prerequisite, is at least an advantage in the prospective analysand. Since psychoanalysis requires a more or less complete reliving of the past and since the older the individual the longer such a review requires, psychoanalysis has had greater success with young than with old people. We have already seen that intelligent conscious control of previously unconscious impulses is the aim of psychoanalysis. Thus intelligence becomes a prerequisite for a successful psychoanalysis. The overcoming of resistance and the establishment of transference is necessary if psychoanalysis is to work. Since this cooperation is almost impossible in deeply psychotic individuals, psychoanalysis has had its greatest success with psychoneurotics.

When one considers all the foregoing, psychoanalysis compares very favorably with the functional therapies of internal medicine.[5] Recent studies (**2**; **3**; **18**; **20**), furthermore, indicate its great importance in the understanding of many of the functional pathologies usually treated solely by internal medicine. Despite the growing importance of psychoanalysis as a specific medical discipline, its value as a psychotherapy remains decidedly limited. There are reasons for believing that all medicine of the future will be more concerned with preventive than with therapeutic problems. I believe the mental hygiene of the future will be concerned more with psychoanalysis than with any of the other schools of medical psychology. Lack of space prevents me from giving my precise reasons for this statement.

We may summarize by stating: *when one keeps in mind the difficulties which confront all psychotherapy and also the ambi-*

[5] I base this statement on the printed reports of the Berlin, London, and Chicago Psychoanalytic Institutes.

tiousness of psychoanalytic therapy, psychoanalysis has been unquestionably successful with certain types of mental disorder. Its efficacy is, however, limited by economic, medical, and psychological considerations.

Relation to Psychology

The past three sections have summarized the methodological adequacy of psychoanalysis and related it to psychology as the science of human behavior. We have seen that it is the "best" method for observing the sources of human action and the most systematic set of concepts concerning the nature of these sources. In this section, as an academic psychologist I wish to outline very briefly how I think psychoanalysis should be related to the training of the academic psychologist.

It would be useless to deny that there is at present a schism between academic psychology and psychoanalysis. Although psychoanalysis is increasingly taught in the neuropsychiatric departments of the medical schools, so far as I know there is no academic department of psychology where a basic training in psychoanalysis is part of the routine curriculum for undergraduate majors and graduate students. Psychoanalysis has fared and continues to fare badly in the universities. Thus the psychoanalysts have been driven to develop their own training centers outside the universities.[6]

This state of affairs works to the disadvantage of both psychoanalysis and the universities. Although psychoanalysis is of the greatest importance to medicine, it is, as I have tried to show in this paper, of even greater importance for the science of psychology. The present psychoanalytic training centers, primarily for psychiatrists, only occasionally have the opportunity to train research psychologists. The teaching staffs are made up almost exclusively of practising physicians, often with little teaching experience. Library and laboratory facilities are usually inadequate; the atmosphere of leisure necessary for scholarly research scarcely prevails in them.[7] And the training centers have hardly any opportunities to develop research programs into problems

[6] Such institutes or training centers are located in the United States in Boston, New York, Chicago, Washington, Baltimore, Topeka, and Los Angeles.

[7] In some of the institutes, particularly that of Chicago, persistent efforts are being made to overcome these difficulties and actually to establish the true university atmosphere.

other than those of psychiatry. Thus psychoanalysis suffers as a branch of psychology by its lack of the research facilities which characterize the first-rate universities. Academic psychology, on the other hand, is not being fertilized by the vital and pertinent questions which psychoanalysis is throwing up on every side. Psychoanalysis has more legitimate implications for the field of social psychology—which for our times is of such importance—than have introspectionism, behaviorism, gestaltism (which are now part of every curriculum) combined. Thus contemporary psychology suffers from a rigidity of academic tradition with regard to psychoanalysis, just as philosophical psychology suffered from the same rigidity of tradition regarding experimental physiological methods seventy-five years ago. The result is an academic psychology which is precise but sterile, and a psychoanalysis which is badly in need of scientific criticism but vital and fruitful.

Both sides are to blame for such a state of affairs. The psychologists object particularly to psychoanalysis as a "cult" or "sect." Probably all great scientific discoveries or schools have had to behave in a "cultish" fashion at first. The psychoanalyst has been forced by the defection of non-analytic individuals whom the public confuses with psychoanalysts, to insist on a strict adherence to psychoanalytic doctrine. The refusal of the universities to give psychoanalysts a proper hearing has forced them to develop their own institutes and societies. The academic psychologists claim it is impossible for them to get training in psychoanalysis and without training they cannot practise it for research purposes. That psychoanalysts are "right" in requiring a didactic psychoanalysis for practitioners follows from reasons set forth in the opening section of this paper. On the other hand, I think training in psychoanalysis has been unduly expensive. To this the psychoanalyst, of course, can reply that in the early days the universities could well have adopted psychoanalysis in their curricula and made it less so.

The psychoanalysts regret that they do not get a fair hearing. That their complaint is well founded may be readily attested by a survey of the most frequently used texts in academic psychology. If psychoanalysis is presented at all, it is presented incorrectly and in an inadequate fashion, naively criticized on impertinent grounds. The psychoanalysts on their side incline to look on

psychologists as men of self-limited vision whose compensations take the form of running rats through mazes and investigating hearing with brass instruments, rather than concerning themselves with vital problems. And so it goes.

If I am right in my opinion that psychoanalysis is the major contribution to psychology made in our times and that it should be part of the basic training of all psychologists, sooner or later its inclusion in the university curriculum will be accomplished. When this occurs, the necessity for the separate psychoanalytic institutes will be overcome. The only question which remains is how the change may best be brought about. To so difficult a problem this paper will attempt no answer. Through it I hope, however, to convince some psychologists of the importance of psychoanalysis as a science and of the consequent importance of cooperation with psychoanalysts.

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SOME ARTICULATIONS BETWEEN PSYCHOANALYSIS AND THE REST OF PSYCHOLOGY

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IT is probably impossible at the moment to say, in any terms that will bear critical examination, what psychoanalysis is. The consequent misunderstandings, which are enormous, derive from the fact that the phenomena are unfamiliar and the terms employed for them inaccurately denotative. Thus from one point of view it is most satisfactory to deny that there is anything legitimately to be called psychoanalysis, but from another such a statement seems (incorrectly) to deny that important advances have been made in the understanding of the human mind by persons using a technique so called. The absurdities to which these merely verbal difficulties may lead in the practical field are illustrated by a report that no one is allowed to "practice psychoanalysis" in Norway without written permission from the appropriate government authority (which permission has not and probably will not be given), and that this activity is defined as "any manipulation of the unconscious." How the unconscious is manipulated, or where it is to be found and how identified, are matters left to the imagination, as indeed they would have to be. Such a situation would be comic except that it both hinders the application of a useful technique to pressing problems and opens the door to possible abuses; for it would probably be possible to show that a person who "manipulated the unconscious"

• It has been suggested by the editor that remarks of the sort here put forth gain in significance when the writer's contact with the field in question is indicated. The following information is offered in compliance with this suggestion: The writer undertook in 1926 about 125 hours of analysis with an analyst analyzed by Otto Rank; this was followed within the next two years by about 30 hours on an experimental basis with a Freudian who has since achieved eminence in the field. The writer's study of emotional conflicts in himself and others goes back to late adolescence, and was greatly deepened and enriched by the analytic experience as well as by subsequent extensive reading and discussion with "analysts" of various persuasions, from sociologists with Freudian background and distinguished psychiatrists to the very promising "child guidance" group, the ingenious but as yet not entirely oriented social workers, a forward-looking minority of ministers, and, of late years, an occasional psychologist. The total time spent in actual laboratory work with subjects (formally and informally, before and after 1926) may be estimated as 4000±1000 hours.

by reminding another where he had left his umbrella, was performing an act indistinguishable from many of the activities of psychoanalysts, and that he was thus liable to punishment.

The phenomena in question are continuous with those of the primary social situation or interview: two persons are conversing. To keep this reduction firmly in mind tends to short-circuit a great deal of unprofitable verbalism about the "practice of psychoanalysis," as Freud himself pointed out many years ago. It should also indicate into what field in our conventional division of scientific labor the phenomena fall: they are unequivocally psychological in character, with the slightly sociological component that is inseparable from experiments having a human subject plus an experimenter. They are not far removed, within the field of psychology, from the mental test—for example, the Rorschach is a close but much simplified analogue—and the association experiments. They have in common with these experiments the element of reduction of the present stimulus situation to as near neutrality as possible, with the aim of observing primarily those reactions which may be thought of as the effects of past stimuli. They go beyond these experiments in being aware that such reactions themselves act as stimuli, and in exploiting the consequences of this fact in the subject's interest.

Those elements which they do not have in common with psychological experiments, they do have in common with such activities as education, salesmanship, and religious and political exhortation, all of which involve awareness—broadly correct even though not scientifically formulated—that attitudes have been formed by past stimulation and may be altered by additional stimulation. They have in common with physiology at the present stage almost nothing, although it is certainly to be hoped that the neurophysiology of attitudes may ultimately be discovered. With pathology they have in common only the observation that an improved attitude often accompanies regression of a pathological state; and with medicine they share only the wish, common to all forms of philanthropy, that the subject may become better adjusted to his conditions of living.

To the basic statement given above there are naturally some qualifications. Most obviously, the content of the conversation is not fortuitous; it concerns the feelings, attitudes, and adjustments of the subject. Important modifications are made with respect

to the social conventions in force during the conversation. The subject places himself under the convention to reveal his internal phenomena verbally as rapidly as he becomes able to do so, and the experimenter places himself under the convention to preserve an objective attitude toward these revelations, without explicit emotional content beyond a mild sympathetic interest. It is also expected that the process will be repeated at intervals for a considerable period, and that the experimenter will apply such verbal restimulation as the experience of himself and others has shown to be adapted to bringing about an adjustive result.

These are the operational factors in the definition of psychoanalysis, considered as an activity. Since any or all of them may be reduced or expanded, just where the boundary is crossed is a futile problem. Thus many an older and younger person has informally drifted into what, looked at from one viewpoint, is a short psychoanalysis; and many psychoanalysts have had short therapeutic relationships with subjects which, seen from a somewhat different viewpoint, they have hesitated—arbitrarily—to call analyses. Good teachers, family physicians, counselors, pastors in all times and places have used these techniques, with good and bad results according to the circumstances and their skill. A therapeutic relationship consciously entered into, with a highly trained analyst in charge and extending over a number of years, is perhaps a sufficiently unique human activity to warrant the special name of psychoanalysis. Yet, at the other extreme, it is possible to use (and soundly) the same psychological principles in everyday social intercourse.

Psychoanalysis as a body of data is an equally uncertain entity. A deal of highly valuable information has been gathered by the techniques described; and psychologists should be slow to declare that, merely because it was gathered by physicians who found their patients' problems insoluble by medical techniques, and gathered moreover while the psychologists themselves were studying physiology, it is therefore not psychological knowledge. We may grant both that the credit belongs to others and that the episode contains a sobering lesson, without denying psychological status to the most impressive body of observations on affective phenomena in existence.

The essential content of these observations, which has been stated before, may be recapitulated as follows: (1) the psycho-

analysts have underscored the concept of drive; (2) they have added that of conflict among drives, which is only now beginning to be explored by a movement that can legitimately be called experimental psychoanalysis; (3) they have considered, somewhat unsystematically to be sure, what things can happen to an organism impelled by conflicting drives—particularly what happens when one drive inhibits the expression of another; and (4) they have demonstrated that in some such cases at least the inhibited drive is not destroyed, but may return to motor expression during periods when the inhibition is itself inhibited or relaxed. Finally, with the help of the anthropologists and sociologists they have begun to say some arresting things about the implications of these principles for the construction of the complicated stimulus situation we call culture. It begins to appear, in short, that only psychological engineering, taking account of affective needs and conflicts of individuals in their inter-association, can make possible a society tolerable to any considerable proportion of its members.

Concreteness may be given to these general observations by a discussion of certain processes regularly encountered in the working out of the psychoanalytic interview; the most important of these are interpretation and transference.

Interpretation *is* psychoanalysis, in the narrow sense. All else is precaution and avoidance, or it is reaction by the subject to the therapeutic stimulation that is being applied to him; interpretation is the constructive aspect of this stimulation. Whether interpretation is suggestion is one of those problems of vocabulary over which psychologists and others dearly love to waste their time. All suggestion, of course, states a point of view, and it has this quality in common with interpretation; and any stimulation, interpretative or non-interpretative, verbal or non-verbal, may be reacted to by the subject and thus become "suggestion." The psychoanalytic process, including interpretation, is of course intended to change the subject's attitudes, and is therefore "suggestion"; so also is straight cognitive teaching, or motor instruction and imitation, as in swimming. But interpretation is not persuasion against the subject's will, or "selling"; a great deal of experience shows that such procedures are either without basic effect or have a contratherapeutic effect. Good interpretation consists in demonstrating what is there; it is no

more and no less "suggestion" to do so than to call attention to the presence of a precipitate in a test-tube after two solutions have been combined.

There is no entirely satisfactory theory of the effect of interpretation. The occurrence of so-called "*aha* experiences" (sudden insights, with affective release) has been noted a good many times in reports on problem solving, but beyond reporting the phenomenon investigators have had little to say. Psychoanalytic interpretation is simply the progressive and constructive eliciting of these experiences in the problem field of the subject's emotional structure in its interaction with his environment. For reasons which again are unknown but are exactly parallel in problem solving and psychoanalytic interpretation, the insight experience is regularly followed by the perception of previously hidden connections, the dropping out of irrelevant attitudes and hypotheses, and the elaboration of more adequate hypotheses and active attempts at solution. The classical analysts have referred to this effect as "making the unconscious conscious": "Here is something about my feelings which I didn't understand before; knowing it, I can attack the problem from a new angle." The process evidently has relationships to Gestalt concepts and to the perceptual reorganizations that have been shown to be important factors in learning.

One of the important types of interpretation consists in the collection of instances of habitual emotional attitudes or behavior with respect to corresponding types of situations. An enumeration of instances, for example, where the subject has responded with hostility to older women or with anxiety to interviews with superiors will ordinarily need no further comment with an intelligent subject; with others, or in cases where the subject seems struggling to formulate a generalization, he may need help to formulate it. Why has the subject himself not become aware of these "modes" in the distribution of his responses? Presumably because he has concurrently acquired an opposing habit, or better an anticipatory response, which threatens him with (imaginal) discomfort should such a generalization emerge; the experimenter, having never acquired the same configuration of anticipatory responses, can perceive regularities which would be painful to the subject.

Much attention has been given to the type of interpretation which involves childhood situations and adjustments; and much perplexity has followed the realization that difficulties often are not dissipated when they are brought into historical relationship with a childhood situation. History *per se* dissipates nothing. There are many adjusted adults who have forgotten childhood maladjustments, just as there are many unadjusted adults who remember them too well. The decisive factor is probably the persistence of the unadjustive attitudes, whatever their source—though it is not surprising that a majority of them derive from the most formative and least critical years. Here is evidently a special case of the common observation that unsupported cognition, the simple act of recognition, is not likely to alter habits noticeably. Such alteration is more likely to require the establishment of new coordinations—in the present instance on the emotional level. Though the mechanism, as already indicated, is obscure, it appears to involve establishment of new motivational connections.

A special case which has been extensively discussed is that of the interpretation of dreams and lapses. These phenomena have been designated, in a somewhat unnecessarily mystical manner, as the "royal road to the unconscious." From the viewpoint of the subject they are perhaps of less value as material for interpretation than other parts of his activities, since he is disposed to dissociate them somewhat from his voluntary attitudes. But from the viewpoint of the psychologist they may indeed give some advance indication of the particular pattern of motives which he is about to encounter. This situation may be said to come about because, due to sleep or preoccupation, countervailing motives have become temporarily weakened, with the result that motives which have been suppressed in the usual state of equilibrium may reach the level of imaginal or even motor expression. An especially instructive intermediate phenomenon is the daydream or fantasy, in which motor sets not too strongly inhibited by counter-sets may find implicit expression accompanied by consciousness of their imaginal character. Naïve subjects sometimes show interesting misconceptions especially of the nature of dream phenomena, projecting them completely in a way exactly analogous to magic; and daydreams may some-

times be distinguished from apparitions only because they are not completely projected. The interpretation "Who had this dream?" is usually sufficient to reverse a too complete projection and redirect attention to the motivations revealed.

The problem of symbolism is closely related to dreaming and to some lapses (in the daydream, direct non-symbolic expression is usually obtained, probably because of the relative weakness of counter-motives). The degree of certainty which can be attached to the proposition that A is a symbol for B is variable, but usually very slight. It depends upon the amount of converging evidence —all of which, despite the rather incomprehensible contentions of some classical analysts to the contrary, must be derived from the observations, not from prior theory. In consequence (since a dogmatically presented wrong interpretation may mobilize more or less irrelevant resistances) interpretations should be formulated tentatively, and should be rather an attempt to arrive cooperatively at an adequate statement of the subject's experience than an answer to his problem. The standard test for the adequacy of an interpretation is the insight experience and the new connections and associations which it initiates; the criterion for an incorrect one is lack of these or of any clear emotional reaction.

The theory of symbolism is a chapter in incidental learning. The symbol A "means"—that is, evokes the same reactions as—the object B, *for a given subject,* because that subject has been subjected to similar stimuli from both and has made the same reactions (perhaps imaginal ones) to these stimuli. Thus we may expect the symbols of different subjects to correspond with their experience (in the broadest sense, including internal experience); and, since experiences are more or less common to different persons, we may expect the resulting symbols to have all degrees of universality. Such an analysis disposes at once of the furious battle which has raged about the universality of symbols such as those for the genitals: a fountain pen "stands for" a penis if the subject in question has thought of it in that way; it might do so for a civilized man strongly sensitized by his sex needs, but would hardly do so for a protected adolescent girl or for a Hopi Indian. Fire, by the same token, might symbolize sex desire in all three.

Transference has been the area of some of the more lurid

disputes about psychoanalysis. It appears, however, to be reducible—with some minor addenda—to nothing more exciting than the proposition that similar (usually internal) stimuli evoke similar reactions. That is, people behave socially about as they have learned to behave. One man opens his analysis with a breast fantasy; it develops that a large part of his activities, including the decision to be analyzed, revolves about the seeking of help from someone thought of as able to give it if willing. Another responds to the interpretation that a series of anecdotes have reaction to authority as their common denominator with an image of the one word "Papa." The classical analyst explains that such cases represent the "carrying over" (transference) of libidinal cathexis from the parental imago to the analyst. The psychologist whose formative thinking has been less isolated will probably prefer to say that imaginal stimuli common to the present and remote association sequences call up the same imaginal and affective responses. He may also generalize by adding that although the parents naturally bulk rather large in such sequences, any figures and any life period may be involved; some drama has always been learned, but what particular drama depends upon the situation and the learner.

Additions to this general scheme may be mentioned; they are, however, perfectly concordant with it. One sometimes finds sequences in which "you" appears to mean "anyone," "the world outside of me." Railings and attacks in fantasy on the part of female subjects against the male genitals are of this nature, and appear to signify no more than general resentment against a deprivation of status felt to be exercised by men against women. This matter, obviously related to the role of symbolism, has been excellently treated in Horney's recent realignment of general theory.

Another familiar situation is the much advertised affective attachment of the subject to the analyst, with its less publicized obverse, viz., apparent personal hostility. Because the forms of such attachment and hostility are again directly related to the needs of the personality involved, the situation thus gives unrivaled opportunity for studying them; their emergence is made possible by the special social conventions set up, in which all impulses present are to be verbally expressed without fear

of counter-attack. Hostility is thus to be regarded as a persistent attitude now released from social suppression, and the problem is to study its dynamics and adjustive function. Attachments, correspondingly, are evidence of a drive toward union on some basis or other, such as need for maternal affection and security, atonement for guilt feelings, the need to master or to be mastered, etc.—all with or without a sexual coloring, just as hostility may or may not be accompanied by an urge toward physical annihilation. The problem of whether transference "is" or "is not" love may be filed with other matters of vocabulary until an adequate operational definition of the latter is forthcoming. It is likely that satisfactory friendships and matings are often achieved on the basis of satisfactions of personality needs of the same sort as those encountered in transference situations—it would be surprising, in fact, if intimate associations were out of relation to the emotional habits of the associates—and the association of sexuality with close emotional relationship seems to be an enriching rather than a necessary circumstance.

Resistance may be thought of as that part of transference—in the broad sense outlined above—which consists of the subject's habitual methods of avoiding imaginal pain (anxiety). Its forms are varied but not unlimited, including such mechanisms as flight, aggression, forgetting, submission, etc. The study of these and their part in the total adjustment pattern—like that of transference in general—is the diagnostic as opposed to the manipulative part of the procedure; i.e., it is the systematic observation of the reaction patterns which have been built up by past stimuli, and from which the nature and intensity of these stimuli can be inferred.

The "problem" of "lay analysis" may usefully be discussed; in the present setting, it takes the form of the question, "Should psychologists undertake 'psychoanalysis?'" It seems clear that if anything should be included in psychology, psychoanalysis should; the study of emotional habits, normal and disturbed, is precisely what is meant by a major fraction of psychology. That academic psychology has kept sedulously away from these major problems must, however, be not only admitted, but deplored as an important reason for its sterility. It follows that a large contingent of "trained" psychologists are quite innocent of any

contact with some of their central problems, if indeed they have not been deliberately trained to deny their existence. But they will certainly be asked, not usually for "psychoanalysis," but for help in emotional as well as other psychological problems; and the question is, therefore, whether the psychologist will attempt that for which the less sophisticated justifiably suppose him to be trained—thereby giving himself a belated training—or give up the ghost at once and admit that he is travelling under false colors and intends to continue doing so. Thus phrased, the relevant issue is whether he is likely to injure his subject by attempting to help.

The most obvious source of danger, and the easiest to guard against, is the possibility of overlooking medical problems. The psychologist should by reason of his background training in the biological sciences have a rather sharper nose for physical involvements than a less highly trained person; but if he wishes to play absolutely safe it is very simple to refer all applicants for psychological help to a physician for check-up and care as a matter of initial routine. Concerning the re-education process itself, it is likely at times to be uncomfortable, but rather unlikely to be dangerous. Among the possible exceptions—inherent in the situation rather than the experimenter—is the possibility of suicide; though this contingency is rare, it is well to cultivate as soon as possible enough imagination to "feel" how the world may look to a person who in his own eyes has only one tie to existence—the psychologist.

As to another possible exception, the release of sexual or other inhibitions, a few guiding principles suffice. The therapeutic situation is set up for the sympathetic study of these and other habit patterns, and of their place in the subject's adjustment, on the verbal level—not for their cultivation, suppression, or demonstration. The release of some emotional energy, while inevitable, is to be neither stimulated nor inhibited (unless it reaches antisocial intensities). Time may be wasted; but, even with a rather incompetent helper, the subject has a much better probability of being helped than of not being helped. And of course there is nothing to prevent the psychologist from getting the best training he can.

It will be noted that the present formulation has not stressed the word "psychoanalysis." This is because in the writer's judgment there is no such "thing" as "psychoanalysis." There is a subject, in the emotional stress engendered by conflicting habit patterns, largely implicit; there is a psychologist, accustomed to think about such topics as learning and frustration, and having a knowledge of and if possible some actual experience with re-educative procedures; and there is an experimental situation, consisting mostly of freedom from irrelevant stimulation. It will not be surprising if from the repeated concurrence of these elements therapeutic habits and formulations arise having some relationship to those that have arisen from similar situations engaged in by psychoanalysts—or teachers, or priests. For there is not a psychoanalytic mind, or an educational mind, or a religious mind; there is simply a mind, concerning the phenomena of which persons with different backgrounds may prefer different vocabularies, but which under any vocabulary is subject to the same laws: the laws—if we can discover them—of psychology.

AN EXPERIMENTAL ANALOGUE OF "REGRESSION" WITH INCIDENTAL OBSERVATIONS ON "REACTION-FORMATION" *

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I

Two extremely interesting experimental demonstrations of "regression" in the rat have already been published (13; 30), and it may therefore seem unnecessary to report another instance of this same phenomenon. On the basis of evidence already available, there can be scarcely any question of the reality of regression in human beings,[1] or, as the two studies just cited show, of the possibility of its occurrence in animals well below man in the evolutionary scale. Since neither of these studies, however, was apparently conducted with an altogether satisfactory understanding of the theory of regression as it has been developed clinically, they have served in certain respects to obscure rather than to clarify the fundamental nature of regression and the conditions of its occurrence. The present study represents an attempt to eliminate some of this ambiguity. Although animal experimentation along these and related lines may eventually have something new and useful to contribute to human psychopathology, that day apparently still lies in the future (24). In the meantime, it would seem to behoove those who look forward to making such contributions to acquire the greatest possible familiarity with those theories and principles that have already been derived from careful clinical inquiry at the human level. If clinical concepts are to be taken over and applied carelessly in animal experiments, the results obtained can be expected to be correspondingly confused and meaningless.

* The empirical findings presented in this paper (except those described in Section IV) were reported first at the 1937 Meeting of the American Psychological Association at Minneapolis (27).

[1] Wells (34; 35) and Taylor (32) have published excellent summaries of the literature on regression in human beings.

Since regression is pre-eminently a psychoanalytic concept and since no one has superseded Freud as a theorist in this field, it is appropriate to turn to him for an authoritative statement of this principle. This writer, it so happens, is not a formal systematist, and it is accordingly necessary to extract his purely theoretical views, piecemeal as it were, from the context of his extensive clinical and popular writings. This is not to say, however, that his theoretical formulations are inexplicit, contradictory, or any less comprehensive and complete than the limitations of logic and empirical observation necessitate in a new and difficult field of inquiry.

In a technical paper that appeared in 1916 (10), Freud introduced the topic of regression as follows:

> When we investigate psychoneurotic conditions, we find in each of them occasion to comment upon a so-called *temporal regression*, i.e., the particular extent to which each of them retraces the stages of its evolution. We distinguish two such regressions—one in the development of the ego and the other in that of the libido. In sleep, the latter is carried to the point of restoring the primitive narcissism, while the former goes back to the state of hallucinatory wish-fulfilment (p. 138).

At a later point in the same article, Freud also distinguished between "the previously mentioned temporal or developmental regression" and what he then termed "topographical regression" (p. 143). In subsequent writings, however, he has made comparatively little use of this latter term, employing instead the term "repression." Writing in 1920, Freud said:

> [In the present discussion] we have not been using the word "*regression*" in its general sense but in a quite specific one. If you give it its general sense, that of a reversion from a higher to a lower stage of development in general, then repression also ranges itself under regression; for repression can also be described as reversion to an earlier and lower stage in the development of a mental act. Only, in repression this retrogressive direction is not a point of any moment to us; for we also call it repression in a dynamic sense when a mental process is arrested before it leaves the lower stages of the unconscious. Repression is thus a topographic-dynamic conception, while regression is a purely descriptive one" (9, 299–300).

As is well known, Freud worked out most of his psychological principles on the basis of observations concerning the sex life of neurotic human beings, but, as in the case of regression, many of his findings have a much wider application. Consequently, it is understandable why it should be in this setting that one finds Freud's most trenchant formulations regarding regression, which,

however, because of the dynamic character of all psychoanalytic concepts, are necessarily intertwined with other principles which must also be given at least incidental consideration.

In a paper that appeared in 1912, Freud (12) presented a detailed statement of his conception of regression in the etiology of neurotic illness and posited that its occurrence or non-occurrence is primarily contingent upon two factors: (a) *fixation* and (b) *frustration*. According to the view developed at that time and subsequently held with relatively little modification, the erotic life (libido) of civilized human beings passes through several more or less discrete stages in its development from the so-called narcissism of infancy to the genital heterosexuality of normal adult life. Because of the continuing pressure of the socializing and educational forces in modern society (plus the changing needs of the child, produced by physical maturation), each of the intermediate stages is successively achieved and then abandoned for the next higher level of adjustment, until the final adult goal is reached. If, however, for any of several possible reasons, this developmental process does not go forward in the usual way and the individual never advances beyond one of the intermediate stages, there results what may be called *absolute* fixation. This is a phenomenon that actually occurs in the lives of some persons and is assumed to provide the basis for the sexual perversions. But since there is here no question of a *return* from a later to an earlier mode of adjustment, the problem of regression is not, of course, involved.

On the other hand, in the lives of persons who do make the prescribed step-by-step transitions in their sexual development (however falteringly), the habits that are acquired and reinforced at each successive stage necessarily have a certain strength or intensity (cathexis) that is more or less enduring; and it is the varying strength of such habits that determines the extent to which *relative* fixation may be said to have occurred at any given point. This phenomenon, in contra-distinction to absolute fixation,[2] is of special significance for the understanding of the

[2] Freud holds that, although sharply distinguishable at the conceptual level, there is in actual practice a continuum between absolute and relative fixations. He says: "There exist, therefore, two extremes—'inhibited development' and 'regression'—and between them every degree of combination of the two factors" (9, 318). (It should be noted, incidentally, that *absolute* and *relative fixation* are not Freudian terms, but their introduction seems warranted as an aid to clear theoretical exposition.)

mechanism of regression; for it is presumably the specific nature and extent of the relative fixations in an individual's life-history which determine the pattern of regression ("choice of neurosis")[3] that will occur if, subsequently, a superimposed mode of adjustment or habit system meets with an insuperable obstacle (i.e., if *frustration* occurs) and if there is relatively little opportunity for further *progression* (i.e., for the development of still other new habits). In other words, the greater the strength of a relative fixation, other things equal, the greater the tendency for regression to occur to the level of that fixation when subsequently acquired habits (in the same impulse-need-system) are thwarted, punished, or otherwise prevented from functioning effectively and bringing the customary satisfaction.

Of the various passages in which Freud has described the principle of regression, perhaps none is more vivid or instructive than the following, which, for additional reasons that will later become evident, is worth quoting in full.

> The second danger [besides that of absolute fixation] in a development by stages such as [that of the libido] we call *regression;* it also happens that those portions which have proceeded further may easily revert in a backward direction to these earlier stages. The impulse will find occasion to *regress* in this way when the exercise of its function in a later and more developed form meets with powerful external obstacles, which thus prevent it from attaining the goal of satisfaction. It is a short step to assume that fixation and regression are not independent of each other; the stronger the fixations in the path of development the more easily will the function yield before the external obstacles, by regressing on to those fixations; that is, the less capable of resistance against the external difficulties in its path will the developed function be. If you think of a migrating people who have left large numbers at the stopping-places on their way, you will see that the foremost will naturally fall back upon these positions when they are defeated or when they meet with an enemy too strong for them. And again, the more of their number they leave behind in their progress, the sooner will they be in danger of defeat (9, 298–299).

The writer goes on to say that a clear conception of the relation between fixation and regression is fundamental to the understanding of the psychoanalytic theory of the neurosis, for, as already indicated, regression or a tendency to regression is assumed to lie at the basis of all symptom-formation. When, under the stress of a need that has been denied its customary form of gratification, regression occurs or is in danger of occurring, anticipation of the penalties that were originally effective

[3] Cf. Freud's paper entitled "Types of neurotic nosogenesis" (12).

in bringing about the required habit progression to this particular level of gratification is, according to psychoanalytic theory, sure to be reactivated;[4] and it is precisely this reactivation of old anticipations of punishment which provides the anxiety that is so constant and so conspicuous a feature of every neurosis. It is for this reason that regression is so likely to be pathogenic. In this connection, Freud says:

> I think that you will be soonest reconciled to this exposition of fixation and regression of the libido if you will regard it as preparatory to a study of *aetiology* of the neuroses. . . . People fall ill of a neurosis when the possibility of satisfaction for the libido is removed from them—they fall ill in consequence of a "frustration," as I called it. . . . This of course does not mean that every frustration in regard to libidinal satisfaction makes everyone who meets with it neurotic, but merely that in all cases of neurosis investigated the factor of frustration was demonstrated (9, 301).

> The libido is [thus] blocked, as it were, and must seek an escape by which it can find an outlet for its cathexis (charge of energy) in conformity with the demands of the pleasure-principle: it must elude, eschew the ego. The fixations upon the path of development now regressively transversed . . . offer just such an escape. In streaming backward and re-"cathecting" these repressed [abandoned] positions, the libido withdraws itself from the ego and its laws; but it also abandons all the training acquired under the influence of the ego. It was docile as long as satisfaction was in sight; under the double pressure of external and internal frustration it becomes intractable and harks back to former happier days (9, 314).

> You see that this escape of the libido under the conditions of conflict [frustration] is rendered possible by the existence of fixations. The regressive cathexis (with libido) of those fixations leads to a circumventing of the repressions [inhibiting anxieties] and to a discharge—or a satisfaction—of the libido, in which the conditions of a compromise have nevertheless to be maintained. By this detour through the unconscious and the old fixations the libido finally succeeds in attaining to a real satisfaction, though the satisfaction is certainly of an exceedingly restricted kind and hardly recognizable as such (9, 315).

In thus retrogressively resorting to earlier modes of gratification, the individual, however, commonly re-encounters *old* anxieties; and it is the coexistence of *both* pain- and pleasure-giving functions in the symptomatic acts of the psychoneuroses that gives them much of their apparent mystery, unintelligibility, and tenacity. The paradox is rendered all the more dramatic by the fact that the individual in whom old sources of pleasure are

[4] The connection between punishment (or threat of punishment) and education (habit progression) has been discussed at length in a previous publication (23). It is here taken for granted and will be given only incidental consideration in the subsequent pages of this paper.

being reactivated is likely to know nothing, consciously, of this aspect of the process and to experience only the painful component. As Freud says, "the person concerned is unaware of the satisfaction and perceives this that we call satisfaction much more as suffering, and complains of it" (9, 319).

Taken, then, in its most general, abstract form, regression—or, to use a somewhat more descriptive term, habit regression—is a relatively simple concept. As the preceding discussion shows, it is merely the converse of habit *progression,* which may be defined as the development of a new mode of need-gratification as a consequence of disruption of or interference with a previously established mode of adjustment. If, after habit progression has occurred, the more recently acquired mode of adjustment is in turn disrupted, one of two results may be expected: (a) still further habit progression or (b) return to an earlier mode of adjustment, i.e., habit *regression.*

In contrast to its conceptual simplicity, regression as it actually occurs in the case of any given human being is likely to be a relatively complicated affair. First of all, the behavior that appears when a given level of adjustment is regressed to is almost never exactly the same as the behavior that occurred previously, before this level of adjustment was abandoned. Changes in physical size and appearance, altered status in the community or family, acquisition or loss of habits not directly affected by the regression, re-arousal of anxieties that were used to produce habit progression, and a number of other factors may so confuse any given instance of regression as to render it almost unrecognizable as such. Yet another complication arises from the fact that in many cases neurotic symptoms appear, not because regression has actually occurred, but because it is merely in danger of occurring. Finally, regression at the human level is not something that ordinarily transpires before the eyes of the clinical observer but has to be reconstructed on the basis of the testimony of the affected individual or of other persons.

From the point of view of exemplifying the concept of regression and of giving it objective specificity, it is, therefore, a real advantage to be able to produce instances of this phenomenon under the controlled and reproducible conditions of the laboratory. Beginnings have been made in the study of experimentally

induced regression in children (1), but such work, whether with children or adults, has severe limitations.[5] Animals can, of course, be much more freely manipulated and modified from the point of view of their basic behavior in an experimental setting than can human beings, and their use in this connection is clearly indicated. At the present stage, however, the usefulness of animals in such research is probably mainly pedagogical, in the sense that it provides a more general opportunity for first-hand contact with clinically discovered phenomena than does the clinic itself and supplies paradigms that may be especially useful as aids to the clearer conceptualization of the psychological principles involved. Special care should apparently be exercised at present not to interpret such experimentation as constituting "tests" of the validity of the concept of regression, or of any other psychological principle derived from clinical investigation at the human level. If comparable findings are obtained in animals lower than man, a certain presumption is established in favor of the validity of the observations at the human level; but, on the other hand, failure to obtain comparable findings does not demonstrate the invalidity of the human observations. It is, therefore, as an "analogue" of human regression that the animal behavior reported in the next section is presented and not as a rigorous, experimental attempt to determine whether the theory of regression, as developed psychoanalytically, is "true or false."

II

During the course of an investigation undertaken for a different purpose, it was discovered that by slightly altering conditions, something analogous to human regression could be produced in the white rat. The apparatus being employed at that time and later taken over without modification for the present study, consisted of a box-like compartment, 5″ x 10″ x 36″, the front side of which was made of glass, with the floor consisting of a grill of transverse stainless-steel rods (0.084″ in diameter), spaced ⅜″ apart. The top was hinged so as to act as a door through which rats could be put into and taken out of the apparatus. At the right-hand end of the compartment thus formed was a brass pedal, 2″ x 4″, hinged at the bottom and normally held

[5] Cf. studies by Hovland and Sears (14), and Siipola (31) on "habit reversions" in conflict situations.

in a vertical position by a spring attached to an arm that extended horizontally in the rear. This arm made an electrical contact that was broken whenever the pedal, which moved very easily, was pressed. By means of a 5,000-ohm, wire-wound potentiometer, the sliding arm of which could be slowly rotated by a constant-speed motor and an electromagnetic clutch arrangement, an alternating current (60 cycles, 115 volts at source) could be impressed on the steel rods forming the grill. This current built up linearly from zero intensity until, at the end of 2.25 minutes, it reached a maximum intensity, after which it remained constant until the pedal was pressed, at which time the potentiometer would re-set (due to release of the clutch) and the current on the grill would drop to zero. As soon as the pedal was allowed to return to normal position, the current would again automatically build up.[6]

When a rat (never before subjected to shock) was placed in this apparatus for the first time and the alternating current was allowed to build up, there was usually an initial period of about 60″ during which the shock appeared to be subliminal. There then appeared minor signs of agitation, such as sniffing at the grill, lifting of individual feet, "sitting up," moving the head to and fro, and so forth. As the current became stronger, the agitation became progressively more violent, culminating in jumping, squealing, biting at the grill, clawing at the walls, and random running about. Under these circumstances the average rat would, through chance, hit the pedal and thereby turn off the shock within three to six minutes after its onset. This first, wholly fortuitous escape from shock created a noticeable tendency for the animal to stay at the pedal end of the apparatus; and when the shock next became liminal,[7] the result-

[6] The mechanical details of this apparatus will be described more fully in a later publication. In order to protect the rat from receiving a tetanizing shock in the event of a marked drop in bodily resistance (as, for example, due to its feet becoming moist with urine), a 10,000-ohm fixed resistance was placed in series with the grill and the leads from the potentiometer. The strength of the current impressed on the grill when the motor-driven potentiometer was at maximum setting was adjustable by means of a second (manually operated) potentiometer of the same specifications, in tandem with the first, which, however, was set so as to give the type of behavior described in the following paragraph and was not changed during the course of the present study.

[7] On the second and for a few subsequent presentations of the shock, the limen seemed to be considerably lowered, agitated behavior being observable as soon as 30 to 40 seconds after the current started to build up. This "sensitization" effect has been studied quantitatively and will be discussed separately.

ing agitation consequently occurred primarily in this vicinity. On the second trial, the animal usually hit the pedal within two to four minutes and very commonly "froze" as soon as it did so, in precisely the position that it happened to be in at the instant when the shock went off (reminding one of the game of "Statue" played by children). On the third presentation of the shock, the amount of random movement that occurred before the pedal was pressed and the shock terminated was relatively limited. Learning, in other words, was fairly precipitous. By the time an animal had had ten presentations of shock, random agitation had virtually disappeared and the pedal-pressing reaction had become prompt and specific. The position typically assumed by the rat in pressing the pedal is shown in Fig. 1B. Between trials, after the pedal habit was well established, the rat would usually sit (often on its haunches facing the pedal) in a position that was expeditious for making the next pedal reaction.

In the early stages of learning, escape from shock by pressing the pedal would often be immediately followed by several additional, highly energetic repetitions of this act, not infrequently accompanied by biting of the metal frame surrounding the pedal. This behavior naturally delayed the shock from building up again. Reactions that seemed to be definitely anticipatory also occurred occasionally, some seconds after the last pedal reaction was made but before the shock had again actually become liminal (as judged by other behavior). These reactions likewise caused the shock-presenting apparatus to re-set at zero, but they were not counted as learning trials. In order for a reaction to count as a trial, a minimum of 30 seconds had to elapse between reactions.

In the present study, five Wistar albino male rats, six months old and experimentally naive, were subjected to ten learning trials daily for three successive days under the conditions just described. These animals will be referred to collectively as the Control Group and designated individually as C_1, C_2, C_3, C_4, and C_5. Complete records of learning performance were kept for each of these rats, but they are of no special relevance here and need not be reproduced.

Five comparable animals, constituting the Experimental Group

FIG. 1B

FIG. 1A

TWO MODES OF ADJUSTMENT TO A COMMON STIMULUS SITUATION (ELECTRIC CHARGE ON GRILL): HABIT A AND HABIT B

Habit A consists of sitting quietly on the hind legs, which has the effect of diminishing the subjective intensity of the shock. Habit B consists of pressing a pedal that causes the charge on the grill to drop to zero.

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(E1, E2, E3, E4, and E5), instead of being treated as were the members of the Control Group, were first submitted to the following procedure. On each of six successive days they were put individually into the apparatus, with the shock-terminating pedal covered with a metal curtain. The shock was then allowed to build up to maximum intensity and remain there for 15 minutes. At the end of this time the shock was turned off by the experimenter and the rat removed from the apparatus. Under these conditions all the animals discovered that if they sat quietly on their hind legs, holding their fore-paws well above the grill, they received comparatively little shock.[8] The result was that after two or three 15-minute sessions in the apparatus, the rats would take this position as soon as the shock became sensible and would maintain it, with only occasional interludes of random activity, until the shock was turned off. This reaction of sitting quietly on the hind legs in order to lessen (though not entirely escape) the shock will be designated as Habit A, in contrast to the pedal-pressing reaction, which will be designated as Habit B.

On the day following the six days of training in Habit A, the animals in the Experimental Group were put into the apparatus, with the metal curtain removed, and the pedal therefore made available to them for the first time. Within ten minutes or less time some of the animals showed enough random activity to result in their pressing the pedal. But some of the animals persisted in Habit A so consistently that they did not press the bar in this length of time and had to have their toes pinched slightly by means of a slender rod that was inserted below the grill, in order to break up the Habit-A type of adjustment and bring about a renewal of random activity. In this way all the animals in the Experimental Group were eventually made to press the pedal, after which Habit B (the pedal habit) was learned fairly rapidly, though not so rapidly as it had been learned by the Control Group. This difference was obviously

[8] The relatively low voltage of the current made this effect possible. If a much higher voltage had been used, such that the total amount of current flowing through the rat would have remained approximately constant regardless of changes in the rat's resistance, then the smaller the area of contact with the grill the greater the concentration, or density, of current in the affected areas and the greater the subjective intensity of the shock. Under the latter conditions, sitting on the hind legs would have been more, instead of less, painful than crouching on all fours.

due to the lingering tendency on the part of the former group to meet the shock situation by means of Habit A, a tendency which was not present in the latter group; but since Habit B was a definitely superior mode of adjustment (permitting the rat to escape the shock completely instead of only partially), it eventually superseded Habit A in all cases. By the end of three days of training in Habit B (30 trials in all), the animals in the Experimental Group were quite as proficient in executing it as were the animals in the Control Group. That is to say, after equal amounts of training in Habit B, the animals in the two groups behaved in an indistinguishable manner in this situation. This fact, nevertheless, does not mean that they no longer differed. As will be shortly noted, the fact that their historical antecedents were different caused them to act quite differently under changed conditions.

To summarize the procedure up to this point, the five animals in the Control Group and the five animals in the Experimental Group were given equal amounts of training in Habit B, with only this difference, that the latter were first required to learn Habit A and were then made to abandon it and *progress* to Habit B, whereas the animals in the former group learned *only* Habit B.

On the day following the completion of training in Habit B, the animals in both groups were put into the apparatus with the pedal accessible as usual, except for the fact that whenever they touched it they received a slight shock from its surface.[9] This, a new element in the situation, created a type of conflict. In order to terminate the mounting grill shock, the rat, in other words, had to submit to at least a momentary additional shock from the pedal. Instead of Habit B now functioning without hindrance, as it previously had, a definite obstacle was placed in the way of its free performance. Under these conditions, the animals in the Control Group continued to execute Habit B for ten trials (cf. Section V). On the other hand, four of the five animals in the Experimental Group promptly "regressed" to Habit A (see Fig. 1A). The difference in behavior in this connection is clearly

[9] The current used to produce this shock was derived from a Muenzinger-Walz constant-current apparatus (29) and was adjusted to 0.03 milliamperes.

shown in Table 1, which, taken with the accompanying legend, is self-explanatory.

TABLE 1

The entries represent the time (in seconds) that the individual members of two groups of rats delayed, after the onset (at zero intensity) of a gradually increasing electric charge on the grill on which they stood, before pressing a pedal to cause this charge to drop to zero, the pedal being also for the first time in their experience electrically charged. It will be noted that the pedal-pressing reaction to the grill shock, referred to as Habit B, persisted in all the rats in the Control Group for all ten trials, despite the "frustrating" shock on the pedal. On the other hand, in four out of five of the animals in the Experimental Group, Habit B was abandoned after one to three contacts with the pedal (a delay of 300 seconds being the criterion of abandonment). Upon abandoning Habit B, the rats in the Experimental Group "regressed" to Habit A.

TRIALS	CONTROL GROUP					EXPERIMENTAL GROUP				
	C1	C2	C3	C4	C5	E1	E2	E3	E4	E5
1	59	87	98	92	95	73	186	149	88	73
2	64	94	116	95	75	45	128	277	300	108
3	75	91	128	90	101	157	300	102		78
4	125	74	124	108	120	300		300		78
5	120	110	121	111	69					63
6	92	134	160	137	86					108
7	123	141	86	118	153					75
8	124	118	121	107	75					84
9	113	90	82	98	182					69
10	120	65	105	118	83					108

III

Before discussing the supplementary observations on "reaction-formation" made in connection with the experiment just described, it is desirable to consider the behavior here reported in somewhat greater detail. It is believed that the basic psychological mechanisms underlying this behavior are comparable in all essential respects to those involved in the phenomenon of regression as it is defined psychoanalytically and that by making this parallelism explicit certain problems can be seen somewhat

more clearly than when viewed only against the complex background of clinical observations.

Upon first encountering a given form of noxious stimulation (grill shock),[10] the rats in the Experimental Group in the present study made a characteristic adjustment to it (Habit A). This adjustment represented a *first fixation*. It was not, however, an entirely adequate solution to the problem, and when a new, more completely satisfactory type of adjustment was made available, the animals (in some instances with a little prodding) fairly quickly *progressed* to it (Habit B). This new adjustment constituted a *second fixation*. Now, however, when an "external obstacle"[11] (shock on pedal) was placed in the way of this latter type of behavior, i.e., when a *frustration* was introduced, the impulse to terminate the grill shock by means of Habit B was thrown into *conflict* with the impulse to avoid the pedal shock. Since the margin of advantage that Habit B had previously had over Habit A was now eliminated (in four of the five animals), Habit A became the preferable (less "painful") mode of adjustment and was reverted to. In manifesting this regressive change, moreover, the behavior of the rats here employed further paralleled the behavior commonly observed in human beings under similar circumstances in that a very elementary type of "symptom" (reaction-formation) also emerged. This feature of their behavior will be returned to subsequently.

The first major point to be emphasized in the present connection is that the regressive behavior here reported was *historically* determined. Since the animals in the Control Group did not abandon Habit B when its previously unhampered execution was interfered with, it follows that the animals in the Experimental Group did so, under externally identical circumstances, solely for the reason that their *past experiences* had been different.[12] In other words, because they had first learned, and later abandoned, a given type of adjustment (Habit A), they were *predisposed* to return to it, in a way that the animals in the Control Group were

[10] This may be thought of as analogous in function to libido (erotic drive) in Freud's discussion of regression in human beings (Section I).

[11] Cf. the quotation from Freud (9, 298–299) in Section I.

[12] It can scarcely be supposed that this dissimilarity of behavior was due to "constitutional" differences. Genetically these animals, coming from a highly inbred strain, were practically identical, and the two groups were, moreover, formed by purely random selection.

not, when the subsequently acquired adjustment (Habit B) was interfered with.[13] As the passages cited in Section I indicate, this view of the historical determination of regression is completely consonant with psychoanalytic theory. Freud has unremittingly stressed the role of prior fixations in the causation of regression, and the term itself carries a similar implication. A person cannot "go back" to something with which he has had no previous experience.

In a challenging paper, Lewin (**15**) has, however, taken the position that regression is not historically determined. He says:

> The theory of regression indicated [here] is entirely unhistorical. According to it, the regressive behavior would occur under the described circumstances even if the person in question were created as an adult Golem. In other words: even a person without childhood who never experienced child-like behavior should regress to child-like behavior very different from what has actually happened in the history of the individual (**15**, 209).[14]

According to this writer, the personality of a normal adult is "more differentiated" than that of a child. This so-called differentiation ("number of sub-parts within the person") tends to break down under "high pressure," i.e., the personality becomes less complexly structured. "In other words: a person under pressure should 'regress' to a more 'primitive' level at least as far as his degree of differentiation is concerned" (**15**, p. 208). Lewin adds that this "primitivation" occurs not because of the continuing latent existence of specific habits or patterns of adjustment (fixations) that have been overt at an earlier point in the individual's life-history, but because the personality is more or less destructured, as a result of a traumatic factor (abnormal "tension," "high pressure," or some other strain within the personality). If, for example, a bunch of toy blocks is arranged so as to form a house and if the house is then pushed

[13] This finding may seem to have an obvious moral for education. If "stable personalities" are desired, then let children be taught only one mode of adjustment to any given need. In other words, "bad habits," if allowed to form, not only have to be overcome before "good habits" can be established, but they more or less permanently weaken the "good habits." Therefore, establish the "good habits" in the first place. Although this is good advice in so far as it is practical, the fact is that human needs and human abilities do not develop hand in hand. The infant has most of the basic needs that the adult has, but he has vastly different potentialities for fulfilling these needs in "good" (i.e., socially approved) ways. He must, therefore, necessarily be allowed to develop and engage, often for months or even years, in habits that are "bad" in the sense that they must ultimately be given up for "better" ones. This is a dilemma that must be kept constantly in mind in the training of children (**23**).

[14] Cf. the discussion in Section I.

over, the resulting un-structured mass of blocks is more "primitive"; this change in complexity of organization represents "regression," even though the blocks had never before been in such a disorganized state. According to Lewin, it is sheer coincidence that the personality of the child and of the adult who has "regressed" is, in both cases, relatively undifferentiated.[15]

In his study of reasoning in schizophrenic human adults, Cameron (3) concludes that the intellectual changes occurring in this type of mental disorder involve simple "disorganization" and "confusion" rather than regression, as many writers currently believe. This conclusion agrees with Lewin's re-definition of regression in a-historical terms, but Lewin's views in this connection are not supported by the main body of clinical observation, nor by the findings of the present, admittedly analogical investigation. Personality de-structuralization, of the kind posited by Lewin, may indeed occur, but it seems distinctly dubious to propose that the behavioral changes of the kind that are commonly referred to as "regressive" and believed to be historically determined also fall into this category.

In addition to the question as to whether regression is or is not historically conditioned, yet another issue arises here. In the two studies cited at the outset of this paper, Hamilton and Krechevsky (13) and Sanders (30) have taken the position that regression represents "a reversion to an earlier, well-established mode of behavior, and persistence in that mode despite the relative inefficiency of that behavior in solving the problem confronting the organism. Further, regression has been assumed to arise when the individual is placed in a strong emotional situation" (13, 238; 30, 494). These writers thus indicate, first of all, their acceptance of the orthodox psychoanalytic view that regression is a function of past experience. Their second point, that the mode of behavior regressed to in any given case is relatively "inefficient," involves assumptions that will be returned to in the next section. For present purposes, major interest attaches to their contention that the *precipitating* cause of regression (as contrasted to the predisposing cause, i.e., fixation) is a "strong emotional situation." This latter proposition

[15] See McGill (16) for a critique of Lewin's theory of the a-historical character of psychological causation in general (especially pp. 530–531). Cf. also Boring (2, Chap. V).

conforms to the popular notion that "mental breakdowns" are caused by severe emotional experiences or "shocks," but it is not in keeping with the analytic view in such matters, according to which the precipitating cause of regression is *frustration,*[16] which in turn is defined as a relatively specific interference with an established mode of need-fulfilment. In other words, "emotional situations" are pathogenic, i.e., lead to regression, only when they constitute such an interference. Failure to make this essential distinction impairs the relevancy of the otherwise admirably designed experiments of the writers cited.[17]

In the experiment reported by Hamilton and Krechevsky, rats first learned that they could obtain food either by turning to the right or to the left at the choice point on a simple T-maze, but that they could do so more quickly (due to a shorter path) by turning to the right. When this *right*-turning habit was well established (occurring in 90 per cent of the trials), conditions were altered so that a *left* turn now resulted in their getting to the food more quickly. The right-turning habit consequently began to disappear and to be superseded by a new left-turning habit. At this stage the animals were divided into a control group and an experimental group, and in the case of the latter animals "a strong electrical shock was administered at a point just before the bifurcation" (13, 234). "Upon introduction of shock the experimental group, *as a group,* showed a decided tendency to revert to its former right-turning behavior" (13, 245). The control group, on the other hand, continued, as might have been expected, to show left-turning behavior with gradually increasing regularity. The shock just before the choice point was regarded in this experiment as providing a "strong emotional situation"; and since it resulted in a reduced tendency to perform

[16] Cf. Section I.

[17] In reporting a series of studies aimed primarily at other problems, Miller and collaborators have assigned frustration its proper role in relation to regression as psychoanalytically conceived. They have secured evidence demonstrating: (a) that interference with a thoroughly established maze habit through non-reward results not only in a retrogressive change from few to many errors (blind-alley turns) but also in a reversion to the particular *patterning* of errors observed early in the initial learning period (19); (b) that interference, through non-reward, with the habit of running down a straight alley for food reinstates an earlier gradient of speed of running (20); and (c) that this type of frustration also produces a return of earlier random activity (sniffing about, grooming, etc.) accompanied, however, by a form of "'energization" (aggression?) that makes this activity more vigorous than it originally was (21). Although the role of frustration in the production of regression is demonstrated admirably in these studies, it is didactically unfortunate that the particular situations employed were of such a nature that the role of fixation was not also clearly illustrated.

a subsequently acquired habit, this change was taken as evidence of the statement that regression occurs "when the individual is placed in a strong emotional situation."

The inadequacy of this analysis is indicated by the experiment reported by Sanders. In her study rats were trained to run down an initial common path, take a U-shaped detour to either the right or to the left, and then come back into a final common path, at the end of which they obtained food. Definite preferences were shown by individual rats for one or the other of the alternative routes, which preferences were assumed to be "unlearned," "ontogenetically lower," possibly "inherited." This "natural tendency" to select a given route was then strengthened by introducing a differential temporal delay in the final common path, consisting of only five seconds if the preferred path were taken but of ninety seconds if the non-preferred path were taken. Training was continued until 36 of 40 trials were "in the direction of the natural tendency." At this point the procedure was reversed, the long delay in the final common path occurring if the previously preferred path were taken and the short delay occurring if the previously non-preferred path were taken. The latter procedure was continued until all animals had altered their behavior to the extent of now selecting the formerly non-preferred path in 36 of 40 trials. At this juncture an electric shock was introduced just before each animal reached the choice point, near the end of the initial common path. As a consequence, all five of the rats used in this study reverted to the originally preferred route, although this route was now not "adaptive by any objective standard" since it continued to involve the longer delay in the final common path. On the basis of this finding, Sanders concluded that "regression has an emotional basis," although a second experiment with the same animals showed that if they were shocked in an unrelated compartment, but immediately before they were put into the maze, no regression occurred. The latter finding leads Sanders to add the qualification that although "regression has an emotional basis, . . . this emotionality must be very closely integrated with the situation" (30, 510).

Martin (17), using the same type of apparatus previously

employed by Hamilton and Krechevsky, has likewise recently reported failure to obtain regression by shocking rats outside the experimental situation proper, thus giving further grounds for the inference, suggested by Sanders' findings, that electrical shock (emotional disturbance) *per se* does not produce this phenomenon.

It is impossible to say precisely why it is that in both the Hamilton and Krechevsky experiment and the Sanders experiment, regression occurs only if shock is administered just before the choice point, not if administered in a spatially unrelated situation; but the present writer entertains a strong suspicion that this fact is in some way dependent upon the character of the *anticipatory reactions* in progress as the animal approaches this point. Anyone who has watched rats run a familiar maze has observed their tendency to hug the wall on the side of their next anticipated turn. Such a tendency is probably one of the best objective measures in animals of what is known in human beings as "intent." It is well established that if a person (especially a child) is punished merely for *intending* to do something, the punishment is quite as effective in inhibiting the intended act as is punishment administered during or after the act. On the other hand, if a person is simply subjected to a painful type of stimulation before a given intention is formed (and if no connection is established through the speech process), such a penalty will have no specific deterring effect. Similarly in the case of rats in a maze: if they are shocked before a given "intention" has formed, i.e., before certain anticipatory reactions are in progress, there will be no inhibition of the act for which the anticipatory reactions are appropriate.[18] Although it is realized that this hypothesis, when applied to the experiments in question, raises certain unsettled issues, it points the way for further investigation and serves as a tentative means of bringing the results of these experiments into line with the conception of regression which holds that its precipitating cause is a *specific frustration* (habit interference) rather than a generalized emotional disturbance.

[18] Evidence presented by Miller (22) in another connection lends strong support for this assumption.

IV

In the experiments just cited, the authors take as one criterion of regression the "relative inefficiency" of the habit that is reverted to. If, as in these experiments, the habit reverted to involves the use of a spatially or temporally longer path to food than does the abandoned habit, it may appear that there is indeed an objective basis for comparing the "efficiency" of the two habits. As various writers have pointed out, however, the concept of "efficiency" always has an ultimately subjective reference. In what sense can a shorter pathway connecting two points in space be said to be "more efficient" than a longer pathway, except in the sense that the margin of gains over losses is greater *for the rat* in one case than in the other? Under the simplest conditions, the loss would be locomotor effort (increased fatigue stimulation) and the gain would be food (reduced hunger stimulation). With amount of food obtained per run (on either path) constant and with the effort exerted unequal, then the short path is, in this *subjective* sense, more "efficient." Theoretically, if none of the factors in this situation was altered, the habitual use of the short path would never be abandoned.

The fact is, however, that at a particular point in both of the investigations under discussion the short path *was* abandoned. How did this change come about? According to the present analysis, it could have occurred only because the margin of gains over losses was altered in such a way as to make it "more efficient," in the subjective sense, for the rat to take the long path. In these investigations the food received at the end of a run remained constant, and the relative lengths of the two pathways remained constant. What changed? As already indicated, it is the present writer's conviction that in order to obtain the effect reported by the authors cited, the short pathway must have taken on anxiety-arousing potentialities (due to the electric shock) which it did not formerly possess and which were not acquired (at least, not in the same degree) by the long path. If such can be assumed to be the case, the advantage in using the short path would thereby have been destroyed, and it would have then become "more efficient" to take the longer path; i.e., the margin of gains over losses would have been reversed, with a corresponding shift in behavior, which was "regressive" but not

"inefficient." Only by arbitrarily restricting interest to the food-getting function can the regressed-to behavior be characterized as "inefficient." Such an atomistic approach is obviously unrealistic and can be pursued to no advantage.[19]

When the results of the experiment described in Section II of the present study were first publicly reported (**27**), Dr. Saul Rosenzweig raised the question as to whether the abandonment of Habit B (pedal pressing) and the return to Habit A (sitting on the hind legs) was really a regression, or simply an "intelligent, adaptive reaction to a change in external circumstances." He emphasized the view that real regression always involves giving up one mode of adjustment that is relatively "good" for one that is "not so good." He summarized his point by saying that had he been a rat in the experimental situation described, he would have done just what the rat did but that he would not have felt that he had "regressed." Dr. Rosenzweig did not, in other words, consider the habit that was reverted to any less "efficient" than the habit that was abandoned, and suggested that the change was not, therefore, genuinely regressive.

The writer fully agrees, as may be surmised from the preceding discussion, that it was *not* less "efficient" for the rats (Experimental Group) that had learned Habit A, as well as Habit B, to revert to Habit A when an electric charge was put on the pedal (thereby interfering with Habit B); but it is not so easy to concede that this change in behavior cannot, for this reason, be legitimately regarded as a regression. As already pointed out, this type of change seems to possess all the essential attributes needed to qualify as regression in the traditional psychoanalytic sense; and it may help in clarifying the issue to recall that one of the distinctive contributions of psychoanalysis has been to break down the dichotomy between "normal" and "abnormal," "adaptive" and "maladaptive." According to psychoanalysis, all behavior—including even the most rampant and maddest of psychoneurotic symptoms—is adaptive in the basic sense that the individual who is doing the behaving is trying to diminish his discomfort and tensions, thereby obeying the all-inclusive "pleasure-principle" (**8**).[20] Living organisms do what they have to

[19] The writer is assuming no one will hold that the regression observed in these experiments had a "structural" basis, i.e., was produced by differential damage to the neural mechanisms mediating the two habits.

[20] *Vide infra*, Footnote 24.

do and can; from the point of view of their own psychic economy every act represents the most "efficient," most "adaptive" behavior that is psychically possible under the circumstances.[21]

It is not difficult to see why there should be a strong inclination to assign a negative value judgment to regressive behavior. Human regressions are commonly in a direction *opposite* to that of the educational, socializing forces of the group; regression, under these conditions, is anti-social (counter-cultural)[22] and as such is regarded *by other persons,* as "bad," i.e., from their point of view "undesirable." This fact may prompt the group to take such retaliatory action toward the regressing individual (social disapproval, segregation, etc.) as will prevent the regressed-to mode of adjustment from being as satisfactory as it otherwise might be; but it is still the "lesser of two evils," from the individual's standpoint. If such were not the case, the regressive adjustment would be abandoned.[23]

If, therefore, in the experiment described in Section II, the mode of adjustment (Habit A) that was regressed to when Habit B (pedal pressing) was interfered with was necessarily "better" from the rat's point of view, and if it is scarcely meaningful to speak of a rat's behavior as being either "good" or "bad" in the social sense (particularly in a situation that did not simultaneously involve other rats), then there would appear to be no grounds for holding that this adjustment was in any

[21] This somewhat dogmatic statement raises a recondite psychological problem. How does one account for such apparent violations of the pleasure-principle as "deliberate" postponement, or even permanent renunciation, of gratifications that are immediately available (in the objective sense) and the self-infliction of injury or the solicitation of such treatment from others? This issue has, however, been dealt with in two other papers (**25**; **26**) and will not be discussed at this time.

[22] Cf. Dollard (**4**).

[23] "Better" adjustments than the one regressed to may, of course, be theoretically possible in a given situation, but the actual behavior of any particular human being is always limited, not by an all-wise Providence, but by that individual's own perceptions, knowledge, and intelligence, i.e., by his existing habit systems and his capacity and opportunity for new learning. (To take an example at the animal level, it was theoretically entirely possible in the conflict situation described in Section II, with electric shock on both the grill and the pedal, for the rat to turn over and lie on its fur-covered back, thereby completely escaping all shock. But this behavior is not in the reaction repertoire of ordinary rats and did not, therefore, come within the range of adjustmental alternatives in this situation.) The essence of many types of psychotherapy consists of an attempt to enlarge the patient's repertoire, or range, of relief-bringing (satisfying) behavior, either by "advice" (instruction) or by placing him in situations in which new learning is demanded (occupational therapy, change of environment, etc.). The psychoanalytic approach, on the other hand, rests on the assumption that most behavior limitations (neurotic incapacities) are due, not so much to simple habit deficits, as to internal conflicts (inconsistent habits) involving anxieties that are not "realistic" (**28**). The therapeutic goal of analysis, therefore, is to remove these anxieties and thereby free the individual for effective, satisfaction-bringing action.

psychologically significant sense inferior, considering the changed character of external circumstances. It was, to be sure, a less-preferred adjustment *before* the shock was put on the pedal, a fact clearly shown by the alacrity with which the rats "progressed" from Habit A to Habit B when the pedal was for the first time made available to them (without shock on it). The mere fact, however, that in one situation a given type of adjustment (Habit A) may be inferior to another (Habit B) does not mean that, with altered circumstances (shock on pedal), the former may not become superior. In the field of *habit dynamics,* functional values are relative, not absolute; and much confusion can apparently be avoided if this fact is kept continually in mind.

In the light of the foregoing discussion, the supplementary experiment, reported below, may seem superfluous—and in the writer's estimation it is, in so far as the validity of this discussion is concerned; but it was nevertheless carried out, mainly as a matter of curiosity, and also partly in the hope that it might further point up some of the issues previously raised. It will be recalled that Dr. Rosenzweig questioned the justification for calling the reversion from Habit B to Habit A in the above experiment a "regression," on the grounds that, with the alteration in external circumstances (introduction of shock on pedal), Habit A, which had previously been abandoned for Habit B, ceased to be an inferior type of adjustment and became the superior adjustment; and going from an inferior to a superior adjustment was not to be regarded as a "regression." The writer's reply has been, in effect, that behavioral changes are always *progressive* in the dynamic sense[24] and that regression has meaning, therefore, only in the *genetic* sense; and since Habit A was genetically prior to Habit B, then the change noted *was* a regression.

The supplementary experiment here referred to was carried

[24] This statement must be qualified in two respects. In the first place, it applies only to behavioral changes that are relatively stable, that involve, as it were, at least a temporary dynamic equilibrium. It obviously does *not* apply to behavioral changes that involve mere random, unorganized, unrewarded striving. (Cf. Valentine, 33, 214). The second point is that although regression is most clearly, unambiguously definable in the genetic sense, it may also conceivably be defined, in the dynamic sense, as a disruption (due to failure of reward or conflict) of a patterned, well-organized type of adjustment to the given situation, with ensuing recourse to random, unorganized behavior. But since random behavior is always a prelude (save in *Gestaltheorie*) to the establishment of a stable habit, regression, defined in this dynamic sense, is also a genetic regression. Cf. Wells' discussion of the *genetic, dynamic* and *social* aspects of regression (35).

out as follows.[25] Five experimentally naive rats were put individually into the apparatus previously described, and the electric shock on the grill (floor) was allowed to build up. The pedal at the end of the apparatus was available from the outset as a means of escape from the grill shock, but it was also kept continuously charged with the same intensity of electric current previously used to produce regression in the experiment already reported. Under these conditions four of the five rats readily acquired the pedal-pressing response (Habit B), despite the presence of shock on the pedal, and executed it with relatively little evidence of conflict for ten trials per day on each of three successive days. At the end of this time the experiment was discontinued, with no indication that the picture would have changed had it been carried on indefinitely. The fifth animal hit upon and then abandoned the Habit-B type of response to the grill shock after two trials on the first day and manifested instead a Habit-A type of adjustment. On the second day this animal completed ten Habit-B responses, but on the third day, after four Habit-B responses, again resorted to Habit A. In short, the results thus obtained show that in four of the five rats tested, the Habit-B type of adjustment was consistently preferred to the Habit-A type of adjustment, under the conditions stated, while in one animal the choice was unstable.

These results indicate, in other words, that for rats with no previous training ("fixation") in the performance of either Habit A or Habit B, the latter is the favored adjustment. This finding might, therefore, be taken as an answer to Dr. Rosenzweig's query as to whether the reversion from Habit B to Habit A under the conditions previously described was indeed regressive, in the sense of involving a change from a "good" to a less "good" habit. Have we not, in the experiment just described, a four-to-one vote in favor of the "goodness" of Habit B, as judged by five "unbiased" rats? Was not, therefore, reversion from Habit B to Habit A in the other experiment an instance of "true" regression, in that the rats which manifested this behavior did something that four out of five other rats, by the testimony of their own behavior, branded as "crazy," i.e., opposed to the self-interest of "right-thinking" rats in general?

[25] The writer is indebted to Dr. Neal Miller for proposing the particular form of this experiment.

The dilemma is easily resolved by making explicit some of the implied steps in this line of reasoning. In the experiment last performed, what was done, in effect, was to try to set up a social standard by which to judge the "normality" of rat behavior in a given situation. Instead of using a *human* social norm, which would have been patently unwarranted, a *rat* norm was established instead; and then the behavior of other rats was compared and contrasted therewith. Since the behavior of these other rats was found to be *different,* it was judged, therefore, as "abnormal," in this case, "regressive." What was overlooked here—but is fortunately well known because of the experimentally controlled character of the situation—is that the life-histories of the two groups of rats were different in one important detail, namely, that one group had been previously fixated on Habit A whereas the other group had not. The characteristically different types of reaction on the part of the two groups of rats in externally identical situations were, therefore, neither more nor less "normal" for one group than for the other.[26] The difference merely shows, again, the force and cogency of the argument previously advanced, that regression is historically conditioned (by the presence or absence of prior fixations) and that it is primarily in the *genetic* sense that this concept has greatest usefulness and meaning.[27]

V

As early as 1908, Freud (6) alluded to the principle of "reaction-formation," yet it is only within relatively recent years that he and other psychoanalytic writers have particularly stressed its

[26] In another study (24) the point has been stressed that apparently the sole basis for calling many types of behavior "abnormal," in both human beings and the lower animals, is that they are simply not understood by the observer. The problem is rendered especially acute in human beings by virtue of the fact that their life histories are so exceedingly variable, and yet these variations are rarely well known to the observer (or even to the individual himself).

[27] It goes without saying, of course, that in the experiments here reported the picture would have been very different had a sufficiently intense shock been used on the pedal to have prevented *all* rats from pressing it in order to escape the grill shock. Naturally, the intensity of the shock used was standardized somewhat below this point. The relation of intensity of shock in this situation to the type of reaction produced further emphasizes the relativity of effects in the study of habit dynamics. In passing it may be mentioned that the nature of the results obtained in a situation of this kind can also probably be significantly influenced by the relative amount of training given in the performance of particular habits (strength of fixation) and, somewhat less certainly, by the age of the animals employed. The possible role of age was only hinted at in some preliminary experimentation and has not been definitely established.

significance. This state of affairs has been part of a more general shift of emphasis in analytic theory, the outcome of which has been to elevate anxiety to a position of pre-eminent importance as "the fundamental phenomenon and the central problem of the neurosis" (11, 111). According to this latter conception, all symptomatic acts are to be viewed as serving in one way or another as "mechanisms of defence" against this phenomenon. Representing a return to a theoretical position tentatively advanced in 1894 (7) and then abandoned, this interpretation was revived by the elder Freud in 1926 (11) and has recently been further elaborated by Anna Freud (5). The latter lists ten "mechanisms of defence," as follows: regression, repression, isolation, undoing, projection, introjection, turning against the self, reversal, sublimination, and reaction-formation. It is therefore in this setting, as one of a variety of related mechanisms, that reaction-formation will be discussed and exemplified.

Like all other psychoanalytic principles, reaction-formation is regarded as pathological only when it functions with unusual force or frequency. In its simplest, least dramatic forms, it involves nothing more, for example, than the tendency on the part of a student who is especially fond of playing bridge to avoid his bridge-playing friends at the time of an impending examination. The paradox is that he avoids them precisely for the reason that he likes them, but the paradox disappears when it is realized that he also fears the consequence of indulging his liking, at least at this particular time. The student's avoidance of his friends under these circumstances thus represents an attempt to minimize the prospect of failing the examination; it is, in short, a "defence" against anxiety. The tendency for an inebriate to make wide detours in order to avoid passing a familiar bar when trying to support a resolution to be temperate is another illustration of the same mechanism.

Of the various passages in which Freud refers to reaction-formation, the following, constituting an incidental comment on the infantile neurosis of a five-year-old boy, sets forth the essential features of this phenomenon perhaps most clearly.

> Thus we have a conflict springing from ambivalence—a firmly founded love and a not less justified hatred, both directed against the same person. His [little Hans'] phobia must be an attempt to resolve this conflict. Such conflicts due to ambivalence are very common; we are acquainted with another typical

outcome of them in which one of the two contending trends, usually the tender one, becomes enormously augmented, while the other disappears. Only the excessiveness of the tenderness and its compulsive character betray the fact that this attitude is not the only one present, and that it is ever on its guard to keep the contrary attitude suppressed, making it possible to construe a train of events which we describe as repression through *reaction formation* (11, 38).

The element of conflict in such a situation emerges, not only from the love-hate ambivalence *per se,* but also from the knowledge (often unverbalized) that overt expression of hostility would result in "loss of love" (punishment, deprivation, isolation, etc.). Anticipation of such a loss arouses anxiety, and in the attempt at "defence" against the latter the most elaborate efforts may be made to conceal and deny the component of hostility. How can the defence be managed better than by continual testimony, in word and act, of affection and solicitude? But the trained eye detects this subterfuge, this intensification of one type of behavior by the very strength of an opposing impulse.[28] Anna Freud describes as follows the form that reaction-formation may take in a little girl who is highly ambivalent toward her mother:

A child who has been aggressive toward her mother develops an excessive tenderness towards her and is worried about her safety; envy and jealousy are transformed into unselfishness and thoughtfulness for others. By instituting obsessional ceremonials and various precautionary measures she protects the beloved person from any outbreak of her aggressive impulses, while by means of a code of exaggerated strictness she checks the manifestation of her sexual impulses [which would be disapproved by the mother no less than aggression] (5, 51).

Particularly common as the basis of compulsive behavior in both children and adults, reaction-formation is not, however, always easy to demonstrate convincingly from clinical material. Although (like regression) relatively simple as an abstract concept, it is likely in any actual case—save in such elementary examples as first given above—to seem complicated and obscure, especially in situations in which other interpretations are logically possible. It is all the more fortunate, therefore, that what seem to be good experimental analogues of this phenomenon as it appears in human beings can be produced by the use of animals in the laboratory.

It has already been remarked that instances of reaction-formation were observed in connection with the study of regression

[28] The Queen in "Hamlet" (Act III, Scene II, Line 240) shows a nice appreciation of this mechanism in her famous remark: "The Lady doth protest too much, methinks."

reported in Section II. It will also be recalled that after the animals in the Experimental Group had acquired Habit A and then abandoned it for Habit B, a shock was put on the pedal involved in executing Habit B; and that the shock, after a few (one to three) repetitions of the pedal response under these conditions, caused Habit B to be in turn abandoned, with a reversion to Habit A. This breakdown of Habit B and regression to Habit A, although relatively precipitous, did not, however, occur without evidence of conflict in these animals. Even more pronounced in the animals in the Control Group, this conflict took the following form. After Habit B was well established in these animals, they customarily sat very near the pedal after pressing it, in readiness for the next presentation of shock from the grill, When, however, shock was put on the pedal, their behavior was noticeably altered. After discovering that the pedal was charged, these animals would frequently *retreat* from the pedal end of the apparatus soon after they began to feel the grill shock, i.e., as soon as they began to have an impulse to press the pedal. In effect, they were thus *running away from the pedal because they wanted to go toward and touch it;* they were, in other words, manifesting a simple but presumably genuine type of reaction-formation. A more precise, though less dramatic, statement of the causal sequence here involved would be that these rats ran away from the pedal because of anxiety, which was aroused by the tendency to touch it. In this sense the reaction-formation was indeed a defence against (flight from) anxiety. But since the grill shock continued to mount, its motivational value soon exceeded that of the anxiety and overcame the rat's "pedal phobia," driving the animal back to the pedal and compelling it to press it.

In other words, in this situation a state of conflict, or "ambivalence," was generated, in which there were recurrently competing impulses, the one an impulse to approach and press the pedal (and thereby escape the grill shock), the other an impulse *not* to press the pedal (and thereby avoid an additional shock from it). In the absence of grill shock, there was no impulse to press the pedal, nor fear of it, and therefore no need to flee from it. As the grill shock mounted, however, this fear of the pedal recurred, and the animal sought to dissipate it by running away from the pedal; but since complete escape was impossible and

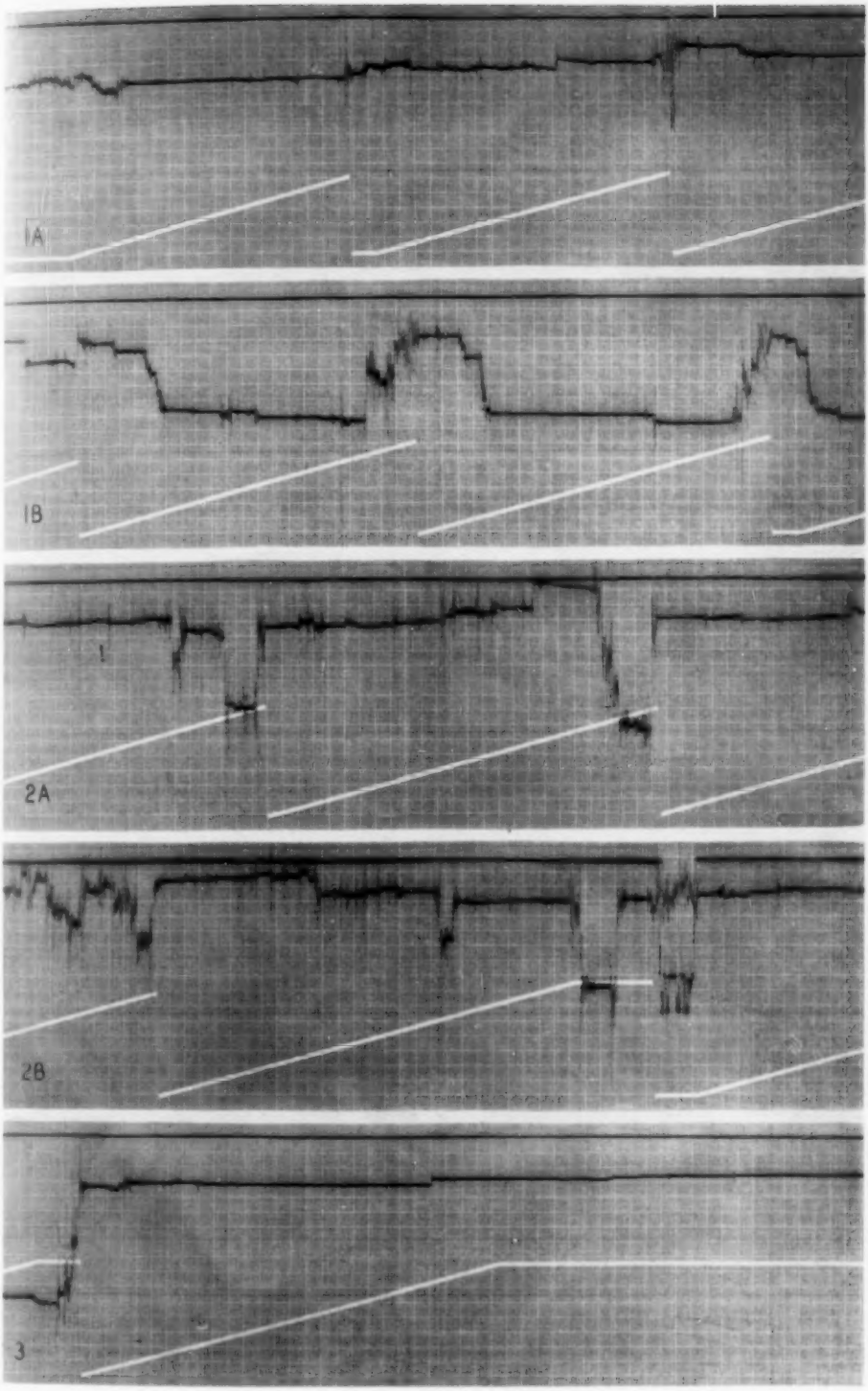

FIG. 2. RECORDS SHOWING INSTANCES OF "REACTION-FORMATION" IN A CONFLICT SITUATION IN RATS

Records 1A and 1B are continuous, as are records 2A and 2B, and were cut only for convenience in mounting.

since the grill shock continued to mount, the fear of the pedal was eventually exceeded by the intensity of the grill shock, and the animal (having no Habit A to regress to) was forced to return to and press the pedal.

The records reproduced in Fig. 2 show some interesting instances of this type of behavior. Before these records can be completely intelligible, however, a brief explanation is due as to how they were obtained. First of all, it should be mentioned that the grill constituting the floor of the apparatus used in this work was "free floating," to the extent that the two metersticks forming the side-rails of the grill were extended approximately another meter at the left-hand end and were there pivoted on a fulcrum, while the right end, not similarly extended, was supported by a system of springs. A thread connected to the right end of the grill communicated with an optical lever which caused a beam of light to register on a piece of moving photographic paper any significant movement on the part of a rat while on the grill. If the rat was at the right end of the grill (near the pedal), the beam of light was deflected toward the left side of the paper (upper side in Fig. 2), and, if the rat moved to the opposite end of the grill, the beam moved to the right (lower) side of the paper. Agitated behavior at any one location on the grill registered simply as rapid transverse oscillations of the beam. The recording system was critically damped to prevent after-vibration. The transverse white lines on the records represent seconds, with five-second intervals accentuated by somewhat wider white lines.

Records 1A and 1B in Fig. 2 are continuous and show the step-by-step development of reaction-formation in one of the animals (C_4) used in the Control Group in the experiment on regression during the session in which shock was for the first time placed on the pedal. It will be noted that the first depression of the pedal, with resulting elimination of the grill shock (represented by the sudden drop of the oblique white lines, indicating grill-shock intensity), occurred without any apparent agitation on the part of the rat (see the irregular black line, made by the light beam controlled by the vertical movements of the grill). The second pedal-pressing reaction was preceded by a slight amount of agitation, as indicated by the oscillations of the light beam

just before the shock-indicator drops to zero. Before the third reaction, the rat moved away from the pedal a little distance (slight drop in the black line at beginning of record 1B), and then, after a pause, returned to the pedal and pressed it. This was the first of a series of responses that seemed to involve unmistakable instances of reaction-formation. As the grill shock built up a fourth time, the rat retreated from the pedal end of the apparatus relatively early and remained at the opposite end until the shock had reached an unusually high intensity, at which point the rat returned to the pedal and, after considerable conflict, pushed it. Almost exactly the same thing happened on the fifth presentation of the shock. Somewhat similar behavior also occurred on the five following presentations of grill shock (not shown).

Records 2A and 2B, obtained from another member (C_3) of the Control Group, show a slightly different pattern of behavior, but the phenomenon of reaction-formation is no less clearly manifested. On the presentation of grill shock first shown in record 2A (which had been preceded by two initial trials), this animal remained at the pedal end of the apparatus until the shock had become quite intense, then fled to the opposite end of the apparatus, remained there in some conflict for eight or ten seconds, but finally returned to the pedal and pressed it without ado. This pattern of behavior is duplicated, with minor variations, on the three subsequent presentations shown in these two records. On the last of these the rat showed considerable intermittent agitation as the shock built up but stayed at the pedal end of the apparatus (save for an incomplete excursion toward the opposite end) until the shock reached maximum intensity (indicated by the flat, non-ascending portion of the white line). At this point the animal dashed to the other end of the apparatus, remained there for a brief period, returned to the pedal, hesitated a while longer, and then finally pushed it. But that action did not end matters. For some reason, perhaps because of the unusual duration and intensity of the grill shock on this trial, the rat continued violently to push and bat at the pedal for a considerable time after the grill shock was turned off. This is indicated by the failure of the grill shock to start building up and also by the break in the black line at the top of the record (made by a

beam of light reflected from a second optical lever attached to the pedal).

Record 3 portrays the behavior of one of the rats (E1) in the Experimental Group at the point when it "regressed." At the extreme left of this record, the grill shock is seen approaching maximum intensity, with the rat at the non-pedal end of the apparatus (indicating reaction-formation). After the shock had reached and maintained maximum intensity for ten or twelve seconds, the animal came, somewhat falteringly, toward the pedal and pushed it. This, however, was the animal's last pedal reaction. It is to be noted that as the next presentation of shock built up to and then remained at maximum intensity, there was not the slightest tendency toward reaction-formation. It was as if the rat had "decided" not to push the pedal again but instead to sit on its hind legs and "take" the shock, i.e., "regress." Since the pedal was dangerous only when there was a temptation to touch it, there was now no reason to fear or flee from it.

These more or less fortuitious observations on reaction-formation are, so far as the writer is aware, the first that have been published concerning this phenomenon in animals.[29] Other instances have undoubtedly been noted in prior experimentation and can probably be multiplied indefinitely by specifically planned attempts to produce them.

SUMMARY

The five major divisions of this paper may be characterized as follows:

Section I reviews the concept of "regression" as it has been developed psychoanalytically and restates it in the terminology of the psychological laboratory.

Section II describes an experiment in which "frustration" of one mode of adjustment, Habit B, caused regression in one group of rats to a previously practiced ("fixated") mode of adjustment, Habit A, but did not lead to regression in another group of rats that had not practiced Habit A before acquiring Habit B.

In *Section III* the hypothesis that regression is "historically conditioned" (by the presence or absence of prior fixations) is

[29] It should, however, be noted that Miller (18) has previously prepared and publicly exhibited a motion picture of reaction-formation in rats.

supported and contrasted to an a-historical interpretation of this phenomenon; the view that "emotional disturbance," rather than specific frustration, is the precipitating cause of regression is also discussed and criticized.

Section IV is devoted to a consideration of the social, dynamic, and genetic implications of regression. It is concluded that from the definitional standpoint the genetic aspect of this phenomenon is basic.

In *Section V* the psychoanalytic concept of "reaction-formation" is defined and exemplified by human illustrations and by behavior manifested by rats in connection with the study of regression.

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HYPNOTIC HYPERMNESIA FOR RECENTLY LEARNED MATERIAL

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An increase in memory capacity is one of the wonders frequently attributed to hypnosis. It has been credited with power to effect the recall of very early childhood experiences, to reproduce in full detail recent incidents which were barely noticed as they occurred, and to clear away stubborn traumatic amnesias. At one time hypnosis was assigned a distinguished place among therapeutic methods because of its apparently exceptional power to relieve symptoms by calling up memories heavily charged with emotion.

Although instances of hypermnesia have been reported with impressive frequency, a certain suspicion has always properly surrounded them. In most cases the original experiences were unknown to the investigator, so that the possibility of fabrication could never be wholly excluded. It was to meet this recurrent objection that Morton Prince (9) began controlled experiments in which subjects were asked to recall letters written some time before but still available for comparison. Prince reported verbatim recall of "fairly long letters" by two of his talented hypnotic subjects, a feat which was quite beyond them in the normal state.

Previous Experiments

During the past fifteen years there have been several experimental investigations of hypnotic hypermnesia. Stalnaker and Riddle (10) showed that hypnosis confers a distinct benefit upon the recall of material committed to memory several years before. The subjects were required to write out what they could remember of "The Village Blacksmith," the Ten Commandments, or similar material memorized during their school years. In forty-six such attempts by a dozen subjects in the normal state, a total of 2,711 words was correctly written, while in an equal number of trials under hypnosis 4,170 words were recalled. The gain

under hypnosis was thus 53.7 per cent of the total waking recall, convincing evidence for the reality and importance of hypnotic hypermnesia.

A contrasting result, however, had been obtained in an earlier experiment by P. C. Young (12) using recently learned material. Adjective-noun associates learned a few days before could be recalled under hypnosis only 5 per cent better than in the normal state, less even than the 7 per cent improvement made by insusceptible control subjects who went through the same hypnotic procedure. In a small additional experiment with two subjects, Young (13) found that hypnosis effected no improvement in the recall of casual observations such as the furnishings of the waiting room, but in one subject slightly and in the other strikingly he obtained hypermnesia for events of early childhood.

Young's findings in regard to recently learned material received ample support from an investigation by Huse (6). The subjects in this case were required to learn paired associates consisting of a nonsense figure presented by an exposure apparatus and a nonsense syllable spoken by the experimenter. Hypnosis was found to be of no benefit to the recall of such material twenty-four hours after the learning; in fact, there was a small advantage in favor of the normal state. In the hypnotic trance there were 546 correct responses, in the normal state 562, giving the latter an advantage of not quite 3 per cent.

PROBLEM

For all their apparent contradiction, these results yield one encouraging uniformity. Both tests in which remote material was employed show marked hypnotic hypermnesia, in agreement with clinical findings, while all tests with recent material give negative results. But the explanatory value of this discovery must be accounted small, for, as Hull (5) remarks, "that hypnosis should produce a lowered threshold for remote memories and no such lowering for relatively recent memories is distinctly paradoxical." Abandoning the threshold hypothesis, Hull sought to explain the apparent importance of the time factor in another way. Remote material is more heavily overlaid with recent acquisitions which stand in the way of its recall. The contribution of the hypnotic state may lie in the weakening of retro-

active inhibition so that the older excitatory tendencies can operate with their full strength. The recall under hypnosis of emotionally blocked material could be explained in closely similar fashion. Recognizing the insufficiency of evidence for these hypotheses, Hull urged an experimental attack upon them.

The present writers, though sympathetic with this proposal, were unable to feel that the main facts of hypnotic hypermnesia were yet before them. In the results so far described, hypermnesia occurred for remote but not for recent material; the time factor seemed to be important. But it is equally true that hypermnesia occurred for meaningful material and not for nonsense; the type of material might be still more important. Positive results were obtained with poetry, prose, and meaningful experiences of childhood; negative with single word associates, nonsense associates, and unimportant items like the furnishings of the room. Unfortunately none of the recent material was meaningful, and since the day is happily past when results with nonsense can be accepted as typical of all human learning, the next step seemed to be an experiment with different types of memory material, holding the time factor constant.

Accordingly we arranged experiments in which hypnotic hypermnesia could be tested with three types of recently learned material. We chose for this purpose (1) paired nonsense associates very similar to the material employed by Huse, (2) poetry more or less comparable to Stalnaker's and Riddle's material, and (3) a series of short moving-picture scenes. We introduced the latter in order to test non-verbal material which was casually observed rather than committed to memory, hence more nearly comparable to the kind of experience for which hypnotic hypermnesia has so often been reported in the clinical literature.

EXPERIMENTAL PROCEDURE

1. *Tests for Recall.* In all cases the subjects were first tested in the waking state. Those who made up the *experimental* group were placed immediately afterwards in the hypnotic state and tested for eyelid catalepsy and catalepsy of the clasped hands; they were then asked to recall the material again. The *control* subjects were allowed an idle period corresponding in length to the hypnotic procedure, about twelve minutes, before making

a second recall in the normal state. Scores are thus expressed in terms of improvement of the second recall over the first, and hypnotic hypermnesia is demonstrated whenever this improvement is significantly greater for the experimental group. This procedure eliminates the difficulty created by individual differences in recall ability and day by day fluctuations in performance, a difficulty which otherwise can be avoided only by using a very large number of subjects and trials.

2. *Depth of Hypnosis.* Before participating in this experiment each subject was tested at least twice for hypnotic susceptibility. The procedure and scoring scale were adapted from Barry, MacKinnon and Murray (1), with certain extensions proposed by Davis and Husband (4). Some of the subjects were rather less susceptible than those reported by Huse and by Stalnaker and Riddle, all of whom could attain a "deep" trance. Our criteria for hypnosis during the memory experiments were inability to open the eyes and inability to unclasp the hands, both of which signify no more than a "light" trance. But there is little doubt that our procedure created deeper hypnosis in those subjects who were capable of it.

3 *Subjects.* For the experiments with memorized material, both nonsense and meaningful, we used eight undergraduate subjects who served first as an experimental group and some days later as a control group. The experiment with casually observed material enjoined upon us the use of different subjects for control, since all the learning, if it were really to be casual, had to be done at a single session. There were five undergraduates in the experimental group, and four in the control.

4. *Nonsense Material.* Following the lead of Huse, we made use of paired nonsense associates for this part of the experiment. We preferred, however, to avoid visual material because it is difficult for "light" trance subjects to open their eyes without awakening. Accordingly we prepared eight sets of nine items apiece, each item consisting of a pair of three-letter nonsense syllables. At each learning session the subject memorized four of these sets. The first syllable of each pair was given printed on a card; after the subject pronounced it, the experimenter spoke the paired syllable. When the set had been run through in this fashion three times, the pack being shuffled to prevent learning

by order, the subject surrendered the cards and the experimenter called out the stimulus syllables, prompting until one perfect performance of the set was attained.

Tests for recall were made 24 hours after the learning. Each set was tested once at each of the two attempts to recall. The number of correct responses at the first attempt, always in the waking state, ranged from 0 to 7 out of the possible nine; the average was 2.9 correct responses or 32 per cent of the material learned.

5. *Meaningful Material.* For the meaningful material we selected passages from the Invocation to *John Brown's Body* by Stephen Vincent Benet, and arranged them to form eight six-line rhymed poems of approximately equal length. At each learning session the subject memorized four poems. He was first permitted to read the poem aloud twice from a printed card, after which he attempted to recite it, being prompted orally until he made one perfect repetition. The experimenter always read as a cue the first line, which therefore was not counted in the results. After the subjects had learned the poetry, they memorized some lists of nonsense syllables for an experiment not reported here. It was on another day altogether that they learned the paired nonsense associates.

The tests for recall took place 24 hours after the learning. Each poem was tested once at each recall trial, the subject being allowed to continue until, after a marked pause, he declared that he could remember no more, and the number of words correctly recalled was taken as the score. At the first attempt, for both groups in the waking state, the number of correct responses ranged from 0 to perfect performance, the latter varying from poem to poem between 33 and 43 words. Since a perfect first score leaves no room for hypermnesia of any kind, we discarded all instances in which the subjects recalled more than 77.7 per cent of the poem learned. This was the highest per cent obtained with the paired nonsense associates, and the two sets of results were thus made more nearly comparable. In Table 3 the complete results are entered, but the discarded figures are enclosed in parentheses and not included in the totals. With this alteration, the average first recall was 11.0 words or 29 per cent of the total material learned.

6. *Moving-Picture Scenes.* Travel pictures taken by one of the experimenters on a trip around the world provided suitable material for this part of the experiment. The films consisted of short unconnected scenes without headings or explanations. As arranged for the experiment, each of two reels contained fifty-seven scenes, and required about eleven minutes to show. In order that the subjects might be fully occupied as they watched the pictures, they were seated in chairs equipped with telegraph keys, and the following instructions were given:

> This is to be a test of alertness and perception. Whenever there are three or more human beings on the screen you are to hold the key down. Otherwise you are to leave the key up. You will see two reels of movies. The first reel was taken in Europe; the second was taken in the Tropics and Far East. Remember, only when there are three or more human beings on the screen are you to hold the key down.

A motor and buzzer were then started in an adjoining room to create the impression that recording devices were in action. Several of the subjects complained that the task was difficult, and in attempting to remember the films could at first think of nothing but three people on the screen. One subject later reported that on going to the movies after this session he found himself pressing down on the arm of the chair every time three or more people appeared on the screen.

Recall of the European film was attempted from one to five days after it was seen, the average time interval for all subjects being 2.5 days. A scene was considered recalled when the experimenter, thoroughly familiar with the film, could recognize it from the subject's description. In practice this crude method of scoring worked fairly enough, as there were very few doubtful cases. The Far Eastern film was used for an experiment in delayed recall not reported here. The atmosphere and subject matter of the two films were so different that in no instance were they seriously confused.

The number of scenes recalled at the first attempt, always in the normal state, ranged from 4 to 18 out of the possible 57. The average was 10.4 scenes or 18 per cent of the total number shown.

7. *Instructions.* We endeavored to use identical instructions for recall in the hypnotic and normal states. For the moving-picture scenes a fairly elaborate and urgent suggestion was given.

In the first attempt at recall the subject was merely asked to "give a list of all the scenes." At the second attempt he received the following instructions:

> Now, I want you once again to think of the first reel of movies which you saw—the ones taken in Europe. You can picture yourself sitting in the darkened room in this building. You can feel yourself sitting in the chair you sat in when you saw the movies. You can feel your finger resting on the telegraph key. Now you can hear the click of the motion-picture projector, and as the movies pass before you on the screen I want you to list all the scenes that you see. These scenes are registered in your mind; you saw them once and you know what they are. If you think hard enough they all come back to you. Now give me a complete list of the scenes. If you think hard enough they will all come back to you clearly and vividly.

These statements, while appropriate enough in the hypnotic trance, evoked looks of surprise when delivered to the wide-awake control subjects, and we could not believe that our "equal" instructions were being received with equal effect. In the experiments with memorized material, therefore, we used the simplest possible suggestion, equally plausible in both states:

> Now, I want you to think of the poetry which you recited to me yesterday. Since you have already tried to recite this once today, it should now come to your mind more easily.

RESULTS

The results for each type of material are shown in detail in Tables 2 to 4, and a summary of the findings appears in Table 1.

TABLE 1

SUMMARY OF THE EFFECT OF HYPNOSIS ON THE RECALL OF THREE TYPES OF MATERIAL

TYPE OF MATERIAL	EXPERIMENTAL GROUP (2nd recall in hypnosis)			CONTROL GROUP (Both recalls normal)			PER CENT GAIN DUE TO HYPNOSIS
	1ST	2ND	PER CENT GAIN	1ST	2ND	PER CENT GAIN	
Nonsense	105	106	0.9	72	84	16.6	—15.7
Poetry	290	487	67.9	304	349	14.8	53.1
Scenes	58	111	91.4	36	39	8.3	83.1

It is immediately apparent in Table 1 that the effect of hypnosis on recall is by no means the same for different types of material. The first column of figures under each group, experimental and

control, shows the total number of correct responses given in a first attempt at recall. This trial was made, alike by both groups, in the normal wide-awake state. The second columns show the total number of correct responses in the immediately following second attempt at recall, which was made by the experimental group under hypnosis but by the control in the same normal state. The percentage of gain is shown in the third columns, and by subtracting the normal gain from that made by the hypnotic group it is possible to arrive, in the right-hand column, at the percentage of gain which may be attributed to hypnosis.

Our findings with paired nonsense associates are in complete accord with the results of Young and of Huse. The hypnotic state is if anything less advantageous than the normal for recalling this type of material. But the situation is altogether different when it comes to meaningful poetry; here we obtained an hypnotic advantage of 53.1 per cent, almost exactly the amount reported by Stalnaker and Riddle for similar material learned several years before. This result annihilates the importance of the time factor and justifies our suspicion that previous investigators by using different types of material created a false dilemma. Hypnotic hypermnesia, it now appears, prevails for meaningful material regardless of the time which has elapsed since learning; but it does not prevail for nonsense. Finally, in the case of casually apprehended moving picture scenes there turned out to be a very large advantage in favor of recall under hypnosis.

The *significance* of the results for nonsense and for poetry can best be weighed in the following manner. The gain made by each subject in the control group is subtracted from the gain which he made on a corresponding piece of material when he served in the experimental group. These differences are given in the extreme right-hand columns of Tables 2 and 3. Since there is no reason, apart from the factor of hypnosis, to expect the average difference between gains to be other than zero, it is possible to calculate the critical ratio which indicates the probability of a true difference between zero and the obtained average. For the paired nonsense associates (Table 2) the average difference, which is in favor of the control group, is only .276, with a standard error of .271, yielding a critical ratio of 1.02.

But with poetry (Table 3) the difference between gains is in favor of the experimental group and amounts to 6.88, which with a standard error of 1.23 gives the very high critical ratio of 5.59. In this case it is necessary to take account of the fact that different poems were used in the control and experimental groups. That no constant error was thereby introduced can be seen by com-

TABLE 2

NUMBER OF PAIRED NONSENSE ASSOCIATES RECALLED IN THE HYPNOTIC AND NORMAL STATES

EXPERIMENTAL GROUP (Second recall under hypnosis)					CONTROL GROUP (Both recalls in normal state)					E-GAIN MINUS C-GAIN
S	SET	1ST	2ND	E-GAIN	S	SET	1ST	2ND	C-GAIN	
Ru.	1	5	5	0	Ru	5	4	4	0	0
"	2	3	3	0	"	6	1	2	1	—1
"	3	4	5	1	"	7	2	2	0	1
"	4	4	4	0	"	8	3	4	1	—1
St.	1	5	4	—1	St.	5	4	3	—1	0
"	2	1	2	1	"	6	3	4	1	0
"	3	3	5	2	"	7	2	3	1	1
"	4	—	—		"	8	2	3	1	
Wa.	1	3	3	0	Wa.	5	1	2	1	—1
"	2	7	7	0	"	5	1	1	0	0
"	3	—	—		"	7	0	2	2	
"	4	—	—		"	8	4	4	0	
Ic.	1	1	2	1	Ic.	5	0	0	0	1
"	2	5	3	—2	"	6	1	1	0	—2
"	3	7	6	—1	"	7	2	4	2	—3
"	4	6	5	—1	"	8	1	1	0	—1
Co.	1	6	4	—2	Co.	5	6	4	—2	0
"	2	3	4	1	"	6	1	3	2	—1
"	3	5	5	0	"	7	3	3	0	0
"	4	3	4	1	"	8	2	2	0	1
Kc.	1	3	3	0	Kc.	5	1	2	1	—1
"	2	2	2	0	"	6	2	2	0	0
"	3	3	2	—1	"	7	3	5	2	—3
"	4	3	3	0	"	8	2	3	1	—1
Ca.	1	0	3	3	Ca.	5	1	1	0	3
"	2	1	0	—1	"	6	1	1	0	—1
"	3	1	1	0	"	7	2	2	0	0
"	4	2	3	1	"	8	2	2	0	1
Hu	1	4	2	—2	Hu.	5	5	6	1	—3
"	2	6	5	—1	"	6	2	2	0	—1
"	3	5	7	2	"	7	5	4	—1	3
"	4	4	4	0	"	8	3	2	—1	1
Totals		105	106	1			72	84	12	—8
Means		3.6	3.6	.03			2.25	2.6	.38	—0.276

paring the average number of words recalled at the first attempt in each session, when the circumstances were identical: the critical ratio for the difference between these averages is only .41. The results thus rather conclusively demonstrate hypnotic hypermnesia for recently learned poetry, while showing that it does not occur when paired nonsense associates are used.

TABLE 3

NUMBER OF WORDS OF POETRY RECALLED IN THE HYPNOTIC AND NORMAL STATES

EXPERIMENTAL GROUP (Second recall under hypnosis)					CONTROL GROUP (Both recalls in normal state)					E-GAIN MINUS C-GAIN
S	POEM	1ST	2ND	E-GAIN	S	POEM	1ST	2ND	C-GAIN	
Ru.	1	27	36	9	Ru.	5	0	1	1	8
"	2	4	8	4	"	6	2	6	4	0
"	3	0	0	0	"	7	2	2	0	0
"	4	5	14	9	"	8	2	5	3	6
St.	1	(34)	(35)		St.	5	0	0	0	
"	2	(36)	(35)		"	6	(28)	(29)		
"	3	(27)	(26)		"	7	(33)	(32)		
"	4	15	27	12	"	8	0	0	0	12
Wa.	1	(33)	(37)		Wa.	5	7	17	10	
"	2	10	22	12	"	6	20	18	—2	14
"	3	22	25	3	"	7	20	19	—1	4
"	4	18	23	5	"	8	28	27	—1	6
Ie.	1	17	26	9	Ie.	5	6	8	2	7
"	2	6	17	11	"	6	15	16	1	10
"	3	0	5	5	"	7	8	9	1	4
"	4	(35)	(35)		"	8	7	16	9	
Co.	1	17	22	5	Co.	5	7	0	—7	12
"	2	8	37	29	"	6	21	19	—2	31
"	3	15	22	7	"	7	17	24	7	0
"	4	18	29	11	"	8	0	14	14	—3
Ke.	1	(38)	(38)		Ke.	5	7	7	0	
"	2	13	18	5	"	6	15	13	—2	7
"	3	(26)	(26)		"	7	19	19	0	
"	4	19	26	7	"	8	13	12	—1	8
Ca.	1	0	3	3	Ca.	5	7	8	1	2
"	2	18	25	7	"	6	12	13	1	6
"	3	9	18	9	"	7	21	27	6	3
"	4	4	12	8	"	8	7	5	—2	10
Hu.	1	19	24	5	Hu.	5	8	9	1	4
"	2	3	11	8	"	6	22	23	1	7
"	3	17	25	8	"	7	(34)	(36)		
"	4	4	12	8	"	8	11	12	1	7
Totals		290	487	197			304	349	45	165
Means		11.6	19.5	7.88			10.5	12.0	1.55	6.88

In the case of moving pictures scenes (Table 4) the data are too limited to established a like certainty. It will be observed that the performance of one subject is largely responsible for the

TABLE 4

NUMBER OF MOVING PICTURE SCENES RECALLED IN THE HYPNOTIC AND NORMAL STATES

EXPERIMENTAL GROUP (Second recall under hypnosis)				CONTROL GROUP (Both recalls in normal state)			
S	1ST	2ND	GAIN	S	1ST	2ND	GAIN
Fr.	17	22	5	Br.	4	5	1
Hm.	11	14	3	Tw.	8	9	1
Ma.	13	46	33	Bl.	6	10	4
Ri.	9	15	6	Di.	18	15	−3
Gu.	8	14	6				
Totals	58	111	53		36	39	3
Means	11.6	22.2	10.6		9.0	9.75	0.75

striking superiority of recall under hypnosis. Since different subjects were used in the two sessions, the measure of significance must be sought in the difference between the two average gains. This difference is 9.85 scenes, with a standard error of 5.17 and critical ratio of 1.9. We claim for this part of the experiment the status of a hint rather than a proof.

Since hypnosis brings about an improvement in recall, it would seem reasonable to expect a positive correlation between depth of hypnosis and amount of hypermnesia. Although we have no measure of depth in the memory experiment itself, it seems probable that the subjects attained such depth as their previous experience and capacity permitted, and this we can infer from our earlier tests of hypnotic susceptibility. To our surprise, we found that the expected relationship does not obtain: the four subjects who showed the most hypermnesia for poetry had an average position of 6.25 in the rank order of hypnotic susceptibility, while the four poorer subjects had the higher average position of 2.75. Furthermore, the subject who made such a striking gain in recalling moving picture scenes ranked next to the lowest in susceptibility in his group of five. These findings suggest the possibility that light trance states are more favorable for hyperm-

nesia than is deeper hypnosis, at least for recent material of the type here employed. This point, however, obviously requires more complete investigation.

DISCUSSION

Our experiments have shown, in accordance with previous results, that hypnotic hypermnesia does not occur for recently learned nonsense. But with the same subjects and essentially the same conditions there is substantial hypermnesia when the material chosen is meaningful poetry. Furthermore, there is some evidence for a similar hypnotic advantage in the case of moving-picture scenes without plot or meaningful sequence.

1. We shall first consider the bearing of these results on the hypothesis that hypnosis benefits recall by lessening the effect of *retroactive inhibition*. This hypothesis is borne out in the present experiment only if it can be shown that retroactive inhibition is considerably greater for poetry than it is for paired nonsense associates. There is in fact one reason to suppose that such might be the case. On the day when the poetry was learned, the subjects immediately afterward committed to memory four lists of nonsense syllables for an experiment not included in this report, while on the day when the paired associates were memorized they left at the completion of the task. It does not seem to us, however, that this circumstance can account for the obtained differences in recall. Previous experimentation, as summarized by Britt (3) and Woodworth (11), has uniformly shown that retroactive inhibition is not great in the case of materials as dissimilar as poetry and nonsense syllables. Even in the work of McGeoch and McKinney (7), where the factor of similarity seemed to make no difference, the amount of poetry retroactively inhibited by nonsense syllables was only five or six per cent. Of much greater importance in the present experiment, where four sets of similar material, poetry or paired associates, were learned at a single session, is the matter of internal retroactive inhibition, and here we can see no reason to suppose that the nonsense fared better than the poetry. There is, on the contrary, some reason to expect exactly the opposite. Among paired nonsense associates which are not even organized into lists there is free play for

mutual interference, whereas the general meaning and structure of the poems serves to give them at least some degree of separateness. Furthermore, the twenty-four hour interval between learning and recall might be expected to inhibit the meaningful material if anything less than the nonsense. In a recent experiment Newman (8) has shown that items necessary to the plot of a story are virtually exempt from retroactive inhibition, whereas irrelevant items suffer it to a marked degree. In view of such considerations we do not believe that our results can be explained in terms of an hypnotic lowering of retroactive inhibition.

2. The next point upon which we wish to focus attention is the exceptional position occupied by *paired associates* in work on recall under hypnosis. Our results with two types of material contribute to the impression that hypnotic hypermnesia is the rule rather than the exception. An enormous mass of clinical instances in addition to the evidence collected by Prince, the experiment of Stalnaker and Riddle, and the performance of our own subjects with poetry and moving-picture scenes, convinces us that hypnosis somehow betters the conditions for successful recall. With a single exception in Young's work, all the evidence *against* hypnotic hypermnesia is derived from experiments with paired associates. On this point a striking uniformity runs through the results of Young, Huse, and ourselves. Is it possible that some special characteristic of paired associates renders them exempt from an effect which is otherwise well-nigh universal? It seems to us that there is indeed such a possibility. In the recall of paired associates the experimenter constantly takes the lead, presenting the first member of each pair and requiring of the subject a single limited response, whereas in all other cases the subject is given an initial cue and then left to recite or elaborate the material without further prompting. Bartlett (2) has proposed that remembering is typically the reconstruction of past experiences out of such images and fragments of "schemata" as are retained. Obviously there is little scope for such reconstruction if the experimenter has already given the whole cue for a strictly limited response. If this be a correct interpretation, the choice by Huse of paired nonsense associates

for a crucial experiment on hypnotic hypermnesia was indeed unfortunate. It is possible that recall can be improved by hypnosis only in so far as opportunity is offered for the active reconstruction described by Bartlett.

3. Finally we come to the most important question of all: *how does hypnosis improve the conditions for successful recall?* Our experiments do not dictate an answer, and it will be well to confine ourselves to certain preliminary remarks. We shall first point out that hypnosis is by no means the only technique which is capable of producing hypermnesia. In a long series of experiments Morton Prince (9) was able to demonstrate recall far in excess of wide-awake capacity by using such aids as abstraction, automatic writing, and crystal visions. Similar effects can be obtained by means of relaxation, twilight sleep, and free association. The common feature of these techniques is the subject's state of relaxation. He does not, of course, abandon his directedness toward the task in hand; it may even be that such directedness is strengthened through his relative indifference to other concerns and, especially in hypnosis, through his desire to please the experimenter. Relaxation itself, nevertheless, has something to do with the betterment of recall. Most people are accustomed to regard alert, volitionally sustained attention as the best of all possible mental states, so that they are surprised to think that mind sometimes works better when left a little to itself. Yet everyone must have had the experience described by James of trying in vain to recall something, only to have it afterwards "saunter into the mind as innocently as if it had never been sent for"; gaps in memory can rarely be filled by simply trying harder. One of our subjects, in an unsolicited introspection, declared that under hypnosis, in contrast to the wide-awake state, the poetry "seemed to flow together nicely." He thereby acknowledged that the material underwent a certain spontaneous reconstruction while he himself remained relatively passive. There are other mental activities, in particular those which we label "creative imagination," which seem to be interrupted rather than assisted by the pressure of an over-alert attention. Scientific hypotheses no less than artistic creations typically come into being by a spontaneous process in which volitional direction, although

it may have set the problem to incubate, stands silently at one side. And if Bartlett is right in his belief that remembering is a creative reconstruction, there is no reason to be surprised that it should share the characteristics of other creative processes.

We do not believe that the hypermnesia obtained by hypnosis is unique for this state, or is solely a product of the special relation between subject and operator. Instead we are inclined to attribute part of the improvement in recall to relaxation, the feature of hypnosis which is common to abstraction, twilight sleep, and numerous other states in which hypermnesia has been found. It would be interesting to parallel our procedure with experiments in which relaxation took the place of hypnosis and suggestion was kept at a minimum. Until then, we doubt the wisdom of further speculation.

SUMMARY

The experiments reported in this paper were devised to test hypnotic hypermnesia for three types of recently learned material. Previous observations and experiments have demonstrated this phenomenon for remote material, but on the basis of experiments with nonsense it has been believed not to occur in the case of material recently learned. We found, in fact, that hypnosis conferred no benefit on the recall of paired nonsense associates learned the day before, but it created substantial hypermnesia (53 per cent) for meaningful poetry, and there was some evidence for a similar gain in the case of moving-picture scenes without captions or plot.

The results cannot be explained by the hypothesis that retroactive inhibition is reduced under hypnosis. We are led to believe that hypnotic hypermnesia is a general rule from which nonsense material is exempt because it allows little scope for that reconstructive activity which Bartlett considers characteristic of remembering. Such activity, like creative imagination, is apparently favored by a relaxed and relatively passive, though still directed, state of mind, this being the factor which is common to all techniques by means of which hypermnesia has been obtained.

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PERSONALITY DIFFERENCES IN ALLERGIC AND NON-ALLERGIC CHILDREN

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"THE term allergy is very generally employed by clinicians who apply it to conditions of specific hypersensitiveness, exclusive of anaphylaxis in lower animals" (7, 4). Examination of medical literature reveals the foregoing definition to approximate most closely the accepted usage. Included among the clinical entities generally conceded to be allergic are asthma, rhinitis, hay-fever, eczema, urticaria, and angio-neurotic edema.

Medical men have for a long time recognized the importance of psychogenic factors in allergic disorders. The literature contains many references both to psychic etiology and to psychotherapy in cases of extreme hypersensitiveness. W. Brauns (4) has reported the successful treatment by hypnotic suggestion of ten out of twelve cases of hay-fever. J. Naber (10) stated that the precipitating factor in asthmatic attacks is psychic and has described successful treatment through psychotherapy. C. D. Doswald and C. Kreiblich (5) produced angio-neurotic inflammation in one patient through hypnosis. In another instance psychoanalysis was found successful in a case of eczema after failure of other methods of treatment, according to Allendy (1). Asthmatic attacks following transfer of a boy to a school in which he was uneasy and unhappy were attributed by Kanner and Lachman (8) to the change in environment. Dunbar has mentioned similar instances in the book, *Emotions and bodily changes* (6).

Few studies have been made, however, of the specific relationship between allergies and psychological traits. Of these, the ones by Balyeat (3), Kling (9), and Thorndike (14) bear most directly on our problem. Using the Otis Self-Administering Intelligence Test, Balyeat studied 80 allergic and an equal number of non-allergic children. Using the Terman classifications for

IQ's, he found that none of the children showing positive intradermal reactions was in the subnormal group as compared with 10 per cent of the control group in the same level. At the other end of the scale Balyeat reported 68.75 per cent of the allergics above normal in contrast to 25 per cent of the control group. Since Balyeat's data are difficult of access, his complete findings are summarized in Table 1.

TABLE 1

BALYEAT'S SUMMARY OF IQ'S OF ALLERGIC AND NON-ALLERGIC STUDENTS

IQ CLASS	ALLERGIC		NON-ALLERGIC	
	N	PER CENT	N	PER CENT
Below normal	0	0	8	10
Normal	25	31.25	52	65
Superior	31	38.75	8	10
Very superior	23	28.75	11	13.75
Near genius	1	1.25	1	1.25

In Carlos Kling's study, a questionnaire composed of 160 items was administered to 108 college students. The items called for responses indicative of neurasthenia, dyspepsia, and allergy. Kling then determined the correlation between the mental and emotional aspects of neurasthenia, the physiological symptoms of indigestion, and the physiological symptoms of allergic disorder. All the coefficients found were below .50 with an average of .42.

More recently Robert L. Thorndike administered an abbreviated form of Kling's questionnaire to 594 college students. The 70 items selected for use were designed to elicit responses indicative of the presence of both allergic and neurasthenic disorder. Analysis of the items to determine the differentiating responses between the allergic and the non-allergic groups revealed some significant differences. Correlations between neurasthenic, symptomatic allergic, clinical allergic, and allied allergic responses ranged from .30 to .60. The average was .45, and all correlations were positive. Here again the possibility of relationship between allergy and psychological disorder is suggested. Thorndike postulated that the tendency of the allergic

toward an increased number of neurasthenic responses is "based more upon feelings of personal inadequacy than upon unsociability and irritability."

The major criticism of the last studies, those of Kling and Thorndike particularly, is that the subjects were not differentiated clinically as to the existence or non-existence of allergies. The division into groups was made on the basis of responses to a questionnaire which included items covering both variables in the experiment. It is conceivable that the neurasthenic tends to imagine the existence of physical symptoms characteristic of an actual allergy.

SUBJECTS AND PROCEDURE

The aim of the present study was to determine whether allergic children differ from normal children in some of the more commonly measured attributes of personality. The investigation departs from those previous to it in that children definitely shown to manifest various clinical allergies were given a standardized personality test and then compared with a control group showing no observable allergies.

The subjects were 139 children, patients in the pediatric allergy clinic of a New York hospital. The children were unselected as to sex and ranged in age from eight to sixteen years. The mean age of the group was eleven years, seven months, with an S.D. of 2.83 years. The nature of the allergy was determined through examination of the medical chart for each child. These charts contained the clinician's diagnosis of the disorder, age of the subject, length of treatment, and number of visits to the clinic. The control group consisted of 117 elementary school children in the fifth to the seventh grades, ranging in age from nine to fifteen years. The mean C.A. of this group was eleven years, six months, with an S.D. of 1.34. Comparison of the mean ages of the experimental and control groups in terms of critical ratios yields a D/S.D. of .32, indicating the absence of any reliable age difference. School medical records were examined to eliminate children diagnosed by the school physician as allergic.

The test used was the Pintner, Loftus, Forlano, Alster *Aspects of personality* (11; 12). The items in this inventory were adapted

from many sources, including the Woodworth-Matthews Psychoneurotic Questionnaire, Pintner's General Opinion Test, Maller's Character Sketches, Allport's A-S Study, and Bernreuter's Personality Inventory. The adaptation took the form of simplifying the wording of the other questionnaires to suit the vocabulary of the fourth grade. The test measures, in three separate sections, ascendance-submission, extroversion-introversion and emotionality (emotional stability). Each section consists of thirty-five items, answered in terms of "I feel the same" or "I feel different." The reliability coefficients are given for the three sections as follows: .73 for ascendance-submission, .61 for extroversion-introversion, and .86 for emotionality.

RESULTS

In discussing the results we shall first pay attention to the scores obtained on the major divisions of the test by the allergic and non-allergic groups, without reference to intra-group differences. Table 2 gives the means, standard deviations, and critical ratios of the total allergic and the control group for the various

TABLE 2

COMPARISON OF CONTROL AND ALLERGIC GROUPS ON THE FORLANO-PINTNER PERSONALITY TEST *

TEST SECTION	GROUP	N	MEAN	S.D.	D/S.D.	CHANCES IN 100
Ascendance-Submission	Allergic	139	61.32	29.15	5.95	99.99
	Control	117	41.05	23.35		
Extroversion-Introversion	Allergic	139	57.89	28.45	1.56	94
	Control	117	51.27	28.70		
Stability-Emotionality	Allergic	139	49.41	32.50	1.95	98
	Control	117	57.21	31.25		

* Increasing scores indicate a trend toward the uppermost in each pair of traits.

attributes of personality tested. Examination reveals that the mean differences between the allergic and control groups are significant in each aspect of the test. The reliabilities of the differences are high, indicating from 94 to 99.99 chances in 100 of a true difference greater than zero. The direction of the

difference shows that the allergics as a group tend to be more ascendant, more extroverted and more emotionally unstable.

In order to determine whether the difference between the allergic and the non-allergic groups was due to the influence of a given type of allergy, the former group was divided into four sub-classes. Included among the "skin allergies" are cases of urticaria, eczema and angio-neurotic edema. The "vasomotor rhinitis" group consists of hay-fever and rhinitis patients. All the asthmatics are in the group so designated. The term "mixed allergy" is used to describe those cases in which more than one type of allergy was manifested. It will be noted that the total number of cases thus described is more than the total for the undifferentiated group of Table 2. This circumstance is the result of the inclusion in Tables 3, 4, and 5 of individuals who have two or more allergies.

The intra-allergy differences in ascendance-submission are found in Table 3. It will be seen that four of the six comparisons are highly significant in that the chances of a true difference

TABLE 3

ASCENDANCE-SUBMISSION SCORES OF ALLERGIC SUB-GROUPS *

ALLERGY GROUP	N	MEAN	S.D.	D/S.D.	CHANCES IN 100
Skin	31	74.45	23.45		
and				2.96	99.98
Asthma	70	58.64	27.95		
Vasomotor Rhinitis	101	61.10	29.45		
and				.55	71
Asthma	70	58.64	27.95		
Vasomotor Rhinitis	101	61.10	29.45		
and				2.59	99.4
Skin	31	74.45	23.45		
Asthma	70	58.64	27.95		
and				1.05	85
Mixed allergy	56	63.91	32.05		
Vasomotor Rhinitis	101	61.10	29.45		
and				.59	73
Mixed allergy	56	63.96	32.05		
Skin	31	74.45	23.45		
and				1.85	96.5
Mixed allergy	56	63.96	32.05		

* Increasing scores indicate increasing ascendance.

greater than zero range from 85 to 99.98 chances in 100. In these comparisons, the skin allergics are shown to be reliably more ascendant than any other group, the mixed allergics next, and the vasomotor rhinitis and asthmatic allergies least of all. Table 4 gives the same data with reference to introversion-extroversion. Here the differences between the various allergic

TABLE 4

EXTROVERSION-INTROVERSION SCORES OF ALLERGIC SUB-GROUPS *

ALLERGY GROUP	N	MEAN	S.D.	D/S.D.	CHANCES IN 100
Skin	31	52.48	34.90		
and				.87	80
Asthma	70	58.64	27.80		
Vasomotor Rhinitis	101	54.17	30.25		
and				.99	83
Asthma	70	58.64	27.80		
Vasomotor Rhinitis	101	54.17	30.25		
and				.24	60
Skin	31	52.48	34.90		
Asthma	70	58.64	27.80		
and				.79	78.5
Mixed allergy	56	54.32	28.25		
Vasomotor Rhinitis	101	54.17	30.25		
and				.02	51
Mixed allergy	56	54.32	28.25		
Skin	31	52.48	34.90		
and				.24	60
Mixed allergy	56	54.32	28.25		

* Increasing scores indicate increasing extroversion.

sub-groups are not nearly as reliable as those in the preceding section of the test. It will be recalled that the reliability coefficient for this section is also the lowest of the three given by the authors. The direction of the differences on this test seems to depart somewhat from that on ascendance-submission. The asthmatic group tends to be more extroverted, whereas the skin group is least so inclined. Since the chances of a true difference are so small, little reliance can be placed on these tendencies. When we turn to the emotional-stability distribution, however, the differences again become statistically significant with only one showing less than 82 chances in 100 of a true difference

greater than zero. Here again, the skin-allergy group tends to be reliably more unstable, with the mixed allergics next in order.

TABLE 5

EMOTIONALITY SCORES OF ALLERGIC SUB-GROUPS *

ALLERGY GROUP	N	MEAN	S.D.	D/S.D.	CHANCES IN 100
Skin	31	32.96	30.15	2.28	98.9
and Asthma	70	48.07	31.60		
Vasomotor Rhinitis	101	47.14	29.45	.19	58
and Asthma	70	48.07	31.60		
Vasomotor Rhinitis	101	47.14	29.45	2.30	98.9
and Skin	31	32.96	30.15		
Asthma	70	48.07	31.60	1.57	93.5
and Mixed allergy	56	39.32	30.50		
Vasomotor Rhinitis	101	47.14	29.45	1.56	93.5
and Mixed allergy	56	39.32	30.50		
Skin	31	32.96	30.15	.93	82.5
and Mixed allergy	56	39.32	30.50		

* Increasing scores indicate increasing emotional stability.

Turning from an analysis of the data in terms of central tendency to the items on the test itself, the formula for the standard error of the difference between two percentages was used to determine the items which were most discriminative between the allergic and the non-allergic groups. The 105 test items studied yielded 21 in which the critical ratio was above 2.00. The items selected by this procedure are given in Table 6, together with the D/S.D. and the chances in 100. It will be noted that there are nine items from the ascendance-submission test, six from the extroversion-introversion, and six from the section on emotional stability.

DISCUSSION AND SUMMARY OF RESULTS

There has been much casual speculation concerning the general personality trends of the allergic individual. This study has attempted to determine quantitatively through the use of a standardized personality inventory those tendencies predominantly displayed by allergic as compared to non-allergic children.

Our test results indicate that the personality of the allergic child tends toward ascendance, extroversion, and emotional instability.

It is usually stated by authorities in the field of personality measurement that certain traits seem to operate in a correlated

TABLE 6

ITEMS DIFFERENTIATING ALLERGIC FROM CONTROL GROUP

ITEM	D/S.D.	CHANCES IN 100
Ascendant responses "Yes" unless noted:		
I talk back to a friend who is busy.	3.0	99.9
I tell the groceryman that it is my turn when the grocer tries to wait on someone else first.	4.1	99.99
I try to get the storekeeper to sell me candy at a cheaper price.	4.5	99.99
I don't mind when other children get in line ahead of me. (No.)	2.2	98.6
I start the fun at a quiet party.	3.4	99.99
I do not like to start a new game among my friends, but I let someone else do it. (No.)	2.8	99.74
I want to lead the class.	4.9	99.99
I like to go from one group of children to another and talk.	2.5	99.4
Extroverted responses "Yes" unless noted:		
I do not like to have people ask me questions about myself. (No.)	2.6	99.5
I keep quiet when I am with other people. (No.)	9.8	100.
I like to spend money.	2.5	99.4
I feel at home at parties.	4.3	99.99
I worry about little mistakes I make. (No.)	2.2	98.6
Emotionally unstable responses:		
I think most children like to make fun of me.	3.3	99.9
I make believe I am somebody else.	3.9	99.9
I often find it hard to breathe.	2.7	99.7
I worry about getting sick.	2.1	98.
I say one thing and do another.	2.3	98.9
I cry when I am in trouble, because then people pity me.	3.8	99.99
Submissive response:		
I like to show people around to meet other people. (Allergic response: No.)	2.6	99.5
Introverted response:		
I make up my mind quickly. (Allergic response: No.)	3.0	99.9

fashion. Stagner (13) reports a correlation of .51 between submissiveness as measured on the A-S Study and emotionality as determined by Thurstone's test. Allport (2) states that "tests for ascendance ordinarily correlate to a certain extent with tests for extroversion (.30 to .40), ratings on strong emotionality with ratings on introversion to an even higher degree." Our results seem, then, to conflict with the findings of others, especially as

to the relationship between emotionality and introversion. In order to ascertain the existence of these conflicting tendencies when the results of normal individuals are compared with those obtained by us on allergics, we have computed correlation coefficients between the three personality traits for both groups of our subjects. The coefficients are presented in Table 7, together with the reliabilities of the differences between the coefficients. It will be noted that all the coefficients are low and

TABLE 7

INTER-TEST CORRELATIONS FOR ALLERGIC AND NON-ALLERGIC GROUPS

TESTS	GROUP	r	P.E.$_{r-r}$	D/P.E.	CHANCES IN 100
Ascendance versus Extroversion	Allergic	.36	.049	1.0	75
	Control	.29	.056		
Ascendance versus Emotionality	Allergic	.005	.056	4.67	100
	Control	—.36	.053		
Extroversion versus Emotionality	Allergic	.25	.053	2.5	95
	Control	.05	.061		

all but one positive. The differences between the two groups of subjects, however, are highly reliable in the case of correlations between introversion and emotionality, and ascendance and emotionality. Here the control group bears out the customary finding of a negative relationship between ascendance and emotional instability, whereas the allergic group shows no such relationship. In the correlation between extroversion and instability, the allergic group shows a significant positive relationship, whereas the control group shows no relationship. In ascendance-extroversion comparisons both groups tend to approximate the usual relationships. It seems, therefore, that allergy tends to be accompanied by personality constellations which differ markedly from those found in normal, non-allergic children. At the present one can but speculate as to the etiology of the differences we have found. One hypothesis that seemed fruitful was that the large age range of the groups studied made for a possible difference. Separation of the groups into school grades, however, showed no consistent trend of increasing or decreasing scores with

changes in school age. Two lines of investigation which stand in need of further research and which we hope to prosecute in the near future will throw light on the questions raised by this preliminary study. The first calls for measurement of personality prior to the first therapeutic treatment and successive tests during treatment, to determine whether clinical improvement is accompanied by changes in personality. The second is the extension of this type of research to the adult allergic.

In summary we may list the following results:

1. Allergic children tend to be more ascendant, extroverted, and emotionally unstable than non-allergics.

2. Correlations between ascendance-extroversion, ascendance-instability, and extroversion-instability are reliably higher for allergics than for non-allergics.

3. Skin allergics are more ascendant and more emotionally unstable than vasomotor rhinitic, asthmatic, or mixed allergics.[1]

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[1] While this study was in press, our attention was called to an article by Chabot, Spadavecchia, and de Sanctis (*Amer. J. Dis. Child.*, 1939, **57**, 831–837) in which techniques parallel to ours were used. The Pintner-Forlano Test was administered to 65 children—20 girls and 45 boys—from an allergy clinic, and the results compared with the norms published for the test. The results showed greater stability for female allergics than for males, both allergic groups having test scores slightly above those for the standardization group. Because of the small number of subjects and the absence of any indices of variability, no critical ratios can be calculated. The apparent contradiction with our results may be accounted for in terms of the possible inclusion of allergics in the original standardization group and also in terms of the selective factors at work in such a small sampling of the allergic group.

SHORT ARTICLES AND NOTES

PEDESTRIAN CONFORMITY TO A TRAFFIC REGULATION

BY STEUART HENDERSON BRITT
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FREDERIKSEN, FRANK, AND FREEMAN have recently published an article concerning the degree of conformity exhibited by 12,666 motorists to a traffic regulation requiring them to keep in line.[1] They report a steep J-curve with 83.3 per cent of the cases falling on the mode. This is the type of curve one would predict from F. H. Allport's hypothesis of conforming behavior.[2]

The present investigation represents a follow-up study of the work of Frederiksen, Frank, and Freeman, in that pedestrians were observed as to degrees of conformity in a type of traffic situation not previously studied in detail.[3] Specifically, the following hypothesis of Allport's was studied in a new pedestrian situation: "In a field of conforming behavior, degrees of conformity are distributed upon their appropriate telic continuum in decreasingly diminishing proportions as one proceeds from the most widely practiced form of the act toward the greatest recognizable deviation from that form."[4]

On March 10, 1939, a set of regulations went into effect in Washington, D. C., designed to control and protect pedestrian traffic. Pedestrians were required to proceed across any intersection controlled by traffic-control signals *only* when the traffic light, situated at the corner to which they were going, showed GREEN (or WALK) and to wait *on the curb* while the light showed RED. For fifteen days prior to the effective date, an intensive "educational" program was carried on through the newspapers, American Automobile Association sound trucks, street-car and bus advertising. Traffic officers stationed at various light-controlled intersections were also engaged in teaching pedestrians what to do and what not to do, and newspaper publicity was given to the fact that the police would follow through with "rigid" enforcement.

PROCEDURE

Since the new law was passed in order to cut down the number of pedestrian injuries and fatalities from automobiles, we chose for study twenty-three intersections in residential areas which had been especially notorious for pedestrian deaths, and four intersections in the downtown area where automobile and pedestrian traffic is very great. A total of 11,339 pedestrians was observed at

[1] N. Frederiksen, G. Frank, and H. Freeman. A study of conformity to a traffic regulation. This JOURNAL, 1939, **34**, 118–123.

[2] F. H. Allport. The J-curve hypothesis of conforming behavior. *J. soc. Psychol.*, 1934, **5**, 141–183.

[3] Miss Miriam Gartner made a study of 124 pedestrians, "but there is an insufficient number of positions to test the J-curve theory adequately." *Ibid.*, 149.

[4] *Ibid.*, 161. Also cf. F. H. Allport, Rule and custom as individual variations of behavior distributed upon a continuum of conformity, *Amer. J. Sociol.*, 1939, **44**, 897–921, 913.

these twenty-seven different intersections.[5] In the residential areas, 3,228 pedestrians were observed at eight intersections in the N.W. section, 2,194 at six intersections in the N.E. section, 1,006 at five intersections in the S.W. section, and 810 at four intersections in the S.E. section. In the downtown area, 4,101 pedestrians were observed at four intersections. This is believed to represent an adequate cross-section of the entire area in which the law applied.

All intersections chosen have a traffic light on *each* corner which flashes a full RED and a full GREEN, so timed that pedestrians may cross relatively easily at the proper time. No intersection was used where the signal was placed in an unusual position, or where the signal lacked a face directly in front of pedestrians crossing toward it, or where there were "trick" arrow movements governing automobile or street-car turns. With the single exception of a special study of 996 pedestrians observed during a 45-minute period at a downtown intersection, no traffic officer was present at the intersections.

The study began the very day the law went into effect and continued over a 50-day period. Observations were ordinarily made on Monday, Wednesday, and Thursday afternoons between 3:00 P.M. and 5:00 P.M., and on Saturday mornings between 9:00 A.M. and 11:00 A.M., although some observations were also made at these same hours on other days.

Four different degrees of fulfillment of the purpose of the prescribed act—steps or positions[6] of Allport's "telic continuum"—were chosen for tabulation. (1) *Waiting on the intersection curb while the light shows RED* represents the maximum degree of fulfillment of the regulation. (2) *Waiting just off the intersection curb* is the next readily distinguishable degree of the purpose of the law.[7] (3) *Proceeding to the middle of the intersection and waiting* is the third position, representing still less satisfaction of the purpose. (4) *Walking across the street disregarding the light,* represents no fulfillment whatsoever of the purpose of the law.[8]

RESULTS

Figure 1 is the distribution curve of the entire 11,339 pedestrians in all areas for the total 50-day period. Of these, 71.0 per cent fell on position 1, which represents complete conformity; 13.8 per cent fell on position 2, representing those who waited just off the intersection curb; 5.4 per cent fell on position 3, representing those who proceeded to the middle of the intersection and waited; 9.8 per cent exhibited complete nonconformity by walking across the street disregarding the light (position 4). The curve is not a typical J-curve.

Is there a difference in the shape of the distribution curve when the data are divided into observations in the residential areas and those in the downtown area? The data plotted in Figures 2 and 3 furnish an answer. In Figure 2,

[5] These observations were made by Mr. Joel B. Hoberman.

[6] "Positions" is probably more accurate than "steps," since a "step" "represents an interval whose equality with the other intervals to the right and left has been established by physical or psychophysical techniques." F. H. Allport, The J-curve hypothesis of conforming behavior. *J. soc. Psychol.*, 1934, **5**, 143, footnote 3.

[7] An unpublished survey by the American Automobile Association indicates that 76.2 per cent of all pedestrians struck by automobiles are struck just off the intersection curb. This fact justifies differentiating this position from position 1.

[8] Allport might well argue that position 4 should be omitted, since the continuum properly contains only recognizable degrees of *fulfillment.* Cf. F. H. Allport, The J-curve hypothesis of conforming behavior, *J. soc. Psychol.*, 1934, **5**, 143, footnote 4. Since the time of the original draft of the present paper, certain additional questions have arisen. Cf. A. Jenness, A critique of the telic continuum and the J-curve hypothesis, *Psychol. Bull.*, 1939, **36**, 531–532; F. Fearing, Conforming behavior and the J-curve hypothesis, *ibid.*, 555.

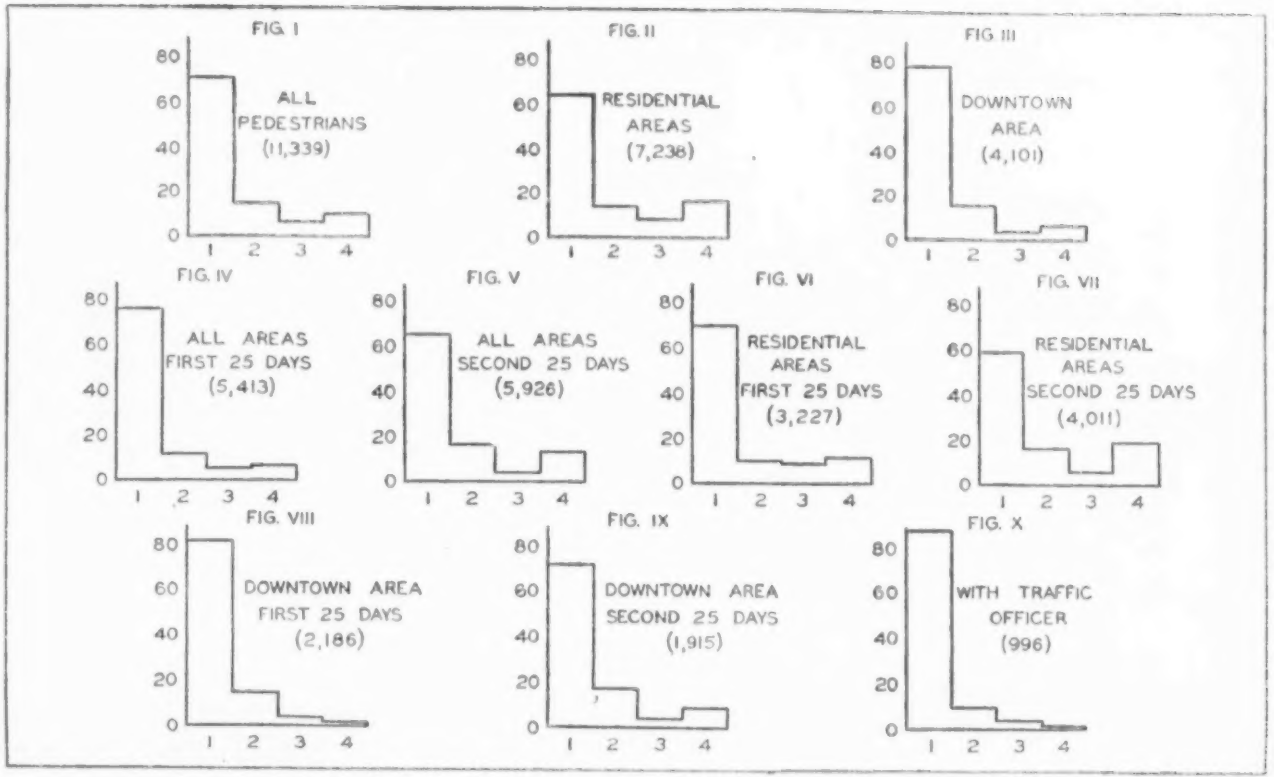
FIG. I
ALL PEDESTRIANS (11,339)
FIG. II
RESIDENTIAL AREAS (7,238)
FIG. III
DOWNTOWN AREA (4,101)
FIG. IV
ALL AREAS FIRST 25 DAYS (5,413)
FIG. V
ALL AREAS SECOND 25 DAYS (5,926)
FIG. VI
RESIDENTIAL AREAS FIRST 25 DAYS (3,227)
FIG. VII
RESIDENTIAL AREAS SECOND 25 DAYS (4,011)
FIG. VIII
DOWNTOWN AREA FIRST 25 DAYS (2,186)
FIG. IX
DOWNTOWN AREA SECOND 25 DAYS (1,915)
FIG. X
WITH TRAFFIC OFFICER (996)
0 20 40 60 80
1 2 3 4

based on 7,238 pedestrians in the residential areas, 64.9 per cent fell on position 1, 12.7 per cent on position 2, 7.7 per cent on position 3, and 14.7 per cent showed complete nonconformity. In Figure 3, based on 4,101 pedestrians in the downtown area, 77.2 per cent fell on position 1, 14.9 per cent on position 2, 2.9 per cent on position 3, and 5.0 per cent showed complete nonconformity. Greater conformity is shown in Figure 3 than in Figure 2, and the distribution curve more nearly approximates the J-curve.

Is there a difference in the shape of the distribution curve when the data are divided into observations during the first 25 days and those during the last 25 days of the 50-day observational period? The data plotted in Figures 4 and 5 furnish an answer. Of the 5,413 pedestrians observed in all areas during the first 25 days, Figure 4 shows that 76.2 per cent showed complete conformity, 11.7 per cent fell on position 2, 6.0 per cent fell on position 3, and 6.1 per cent showed complete nonconformity. Of the 5,926 pedestrians observed in all areas during the last 25 days, Figure 5 shows that 66.0 per cent showed complete conformity, 15.8 per cent fell on position 2, 4.6 per cent fell on position 3, and 13.6 per cent showed complete nonconformity. Figure 4 is almost the J-curve.

These data are further subdivided in Figures 6, 7, 8, and 9. Figure 6, based on 3,227 pedestrians in the residential areas during the first 25 days, shows that 70.1 per cent fell on position 1, 9.4 per cent on position 2, 9.3 per cent on position 3, and 11.2 per cent on position 4. Figure 7, based on 4,011 pedestrians in the residential areas during the last 25 days, shows that 59.8 per cent fell on position 1, 15.9 per cent on position 2, 6.1 per cent on position 3, and 18.2 per cent on position 4. Figure 8, based on 2,186 pedestrians in the downtown area during the first 25 days, reveals that 82.3 per cent fell on position 1, 14.0 per cent on position 2, 2.6 per cent on position 3, and 1.1 per cent on position 4. Figure 9, based on 1,915 pedestrians in the downtown area during the last 25 days, reveals that 72.2 per cent fell on position 1, 15.7 per cent on position 2, 3.1 per cent on position 3, and 9.0 per cent on position 4. Of these four figures it is obvious that Figure 8 is the only typical J-curve.

The final graph, Figure 10, presents the results of a special study of pedestrians *with a traffic officer present*. Observations were made of a total of 996 pedestrians for a 45-minute period during the afternoon "rush" hour at a downtown intersection 48 days after the law went into effect. Figure 10 shows that 89.8 per cent of the persons showed complete conformity, 7.9 per cent fell on position 2, 2.0 per cent fell on position 3, and 0.3 per cent fell on position 4. This curve of distribution, like that shown in Figure 8, is also a J-curve. These are, in fact, the only typical J-curves found.

INTERPRETATION

Of the ten graphs, Figure 4 approximates the J-curve, and Figures 8 and 10 are typical J-curves. Why are these curves of a different shape from those in the other seven figures? Several conformity-producing factors account for these differences.

First, there is the factor of downtown area, or perhaps, more accurately, those factors operating particularly in the downtown area. Important factors in the downtown area as compared with the residential areas are no doubt (a) the greater amount of traffic, and (b) the greater probability of the presence or sudden appearance of a traffic officer.

That conformity is consistently greater in the downtown area than in the residential areas is revealed in a comparison of Figures 2 and 3. Complete conformity is 12.3 per cent greater and complete nonconformity is 9.7 per cent

less in the downtown area than in the residential areas. Similarly, a comparison of Figure 6 with Figure 8 shows that complete conformity is 12.2 per cent greater and complete nonconformity is 10.1 per cent less in the downtown area than in the residential areas (during the first 25 days). Likewise, a comparison of Figure 7 with Figure 9 reveals that complete conformity is 12.4 per cent greater and complete nonconformity is 9.2 per cent less in the downtown area than in the residential areas (during the second 25 days). Figure 3 (downtown) approximates the J-curve much more than Figure 2 (residential); Figure 9 (downtown) approximates the J-curve more than Figure 7 (residential); and Figure 8 (downtown), as compared with Figure 6 (residential), actually is the typical J-curve.

Second, there is the factor of newness of the law following an "educational" campaign; that is, the probability of the presence or appearance of a traffic officer who would actually enforce the law was much greater during the early days of its operation. A comparison of the first 25 days of the law with the second 25 days reveals in all instances much greater conformity during the earlier period. Figure 4 compared with Figure 5 shows 10.2 per cent greater complete conformity and 7.5 per cent less complete nonconformity. Figure 6 compared with Figure 7 shows 10.3 per cent greater complete conformity, and 7.1 per cent less complete nonconformity. Figure 8 compared with Figure 9 shows 10.1 per cent greater complete conformity and 7.9 per cent less complete nonconformity. Figure 4 is almost the J-curve; and Figure 8, with the two factors of downtown area and newness of the law both operating, is a perfect J-curve.

The fact that Figure 1 is not a J-curve may be accounted for by the fact that the data which make up this graph are for *all* conditions, both greater conformity-producing and greater nonconformity-producing. With a mixture of the data on downtown and residential pedestrians, plus a mixture of data on newness of the law with "oldness" of the law, theoretically a non-J-curve would be predicted; and a non-J-curve is actually what is found.

Third, there is the factor of the presence of a traffic officer, a visible (and audible) symbol of law enforcement. Figure 10, based on data with a police officer present, shows the greatest amount of complete conformity (89.8 per cent) and the least amount of complete nonconformity (only 0.3 per cent) of any figure.[9] Figure 10 is also, like Figure 8, a perfect J-curve. It has already been indicated that the factors of downtown area and newness of the law may be partially explained in terms of this factor of law enforcement. Thus, the much greater conformity to the regulation in the downtown area than in the residential areas might be due in part to the greater concentration of traffic officers in this area and the awareness on the part of pedestrians of the possibility of the sudden appearance of this conformity-producing agent. Similarly, the much greater conformity to the regulation during the earlier period of operation of the law than during the later period might be due in part to an awareness on the part of pedestrians of the possibility of rigid enforcement of the law, especially just after an intensive fifteen-day "educational" campaign in which traffic officers actually did train pedestrians in the law which was to become effective.[10]

The size of position 4 in many instances may be partially due to two additional factors. The first of these is the possibility that many of the complete nonconformers were not licensed to drive automobiles, and, therefore, would

[9] In this connection cf. observations of automobile drivers reported by R. T. LaPiere and P. R. Farnsworth in *Social Psychology*, New York: McGraw-Hill, 1936, 400, note. 4.

[10] From March 10, the date the law went into effect, until July 22, only 145 arrests had actually been made for violation of the law.

be less likely to follow traffic regulations than other pedestrians who were also at times drivers of automobiles. A second possibility is that many of the complete nonconformers were non-residents, and, therefore, would be less likely to follow traffic regulations than other pedestrians who were residents of the District of Columbia.

Accordingly, 63 persons who had crossed an intersection in the downtown area in complete disregard of the RED light were asked the following two questions:

"Pardon me. I am conducting a poll of the number of pedestrians who have District driving permits. You need merely tell me whether or not you are licensed to drive in the District. [Following their answer:] Are you a resident of the District?"

In answer to question one, concerning a driving permit, 4 of the 63 refused to answer, 7 answered that they did have a permit, and 52 (82.5 per cent) answered "No." In answer to question two, concerning residence in the District, all 63 answered "Yes." These results suggest that the second possibility, namely non-residence, is probably not important in accounting for the amount of complete nonconformity represented in position 4. These results do suggest, however, that the first possibility—namely, that some were non-drivers of automobiles—may well be an important factor in helping to explain the extent of complete nonconformity (position 4).

SUMMARY

The frequencies of occurrence of acts were plotted against F. H. Allport's "telic continuum," which represented degrees of satisfaction of the purpose of the following traffic regulation: pedestrians were required to proceed across any intersection controlled by traffic-control signals *only* when the traffic light, situated at the corner to which they were going, showed GREEN (or WALK) and to *wait on the curb* when the light showed RED. The curve representing the actions of the 11,339 pedestrians observed in this study had 71.0 per cent cases on position 1 (complete conformity), 13.8 per cent on position 2, 5.4 per cent on position 3, but 9.8 per cent on position 4 (complete nonconformity). This is not a typical J-curve, probably because of a mixture of the data on downtown and residential pedestrians, together with a mixture of the data on newness and "oldness" of the law.

Analysis of these data by plotting other curves suggests that conformity-producing factors in this situation are: downtown area—that is, greater amount of traffic in the downtown area than in the residential areas, and greater probability of the presence or sudden appearance of a traffic officer; and newness of operation of the law—that is, the greater probability of the presence or appearance of a traffic officer during the early days of the operation of the law who would actually enforce it. An additional curve, based on 996 cases, suggests that a strong conformity-producing factor is the actual presence of a traffic officer. As indicated, it is likely that the first two factors may be partially explained in terms of the *possible* operation of the third factor, namely, the possible appearance of a traffic officer.

Curves based on these three factors, alone or in combination, either approximate J-curves or else actually are perfect J-curves.

A special study of 63 persons suggests that the complete nonconformity of pedestrians in the present situation may perhaps be accounted for in part by the high percentage of non-drivers of automobiles among this group.

THE ATTITUDES OF VARIOUS GROUPS TOWARD BEHAVIOR PROBLEMS OF CHILDREN *

BY CHARLES E. THOMPSON
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One of the prime purposes of this study was to discover, if possible, whether teachers judge behaviors on a social basis or from a mental hygienic point of view. In order to carry out this purpose, rankings of behavior problems made by teachers, apprentice teachers, parents, and children, both white and negro, were compared with rankings of the same behaviors made by child psychologists. In order to determine the viewpoint of the teachers, their rankings were compared with those of the child psychologists, who were assumed to represent the mental hygienic point of view, and with those of the parents and children, who were assumed to represent the social point of view.

The same list of problems, simply named but not defined, was presented to these various negro and white groups of teachers, apprentice teachers, and parents. Although the child psychologists expressed the need for a precise definition of the named behaviors, not a single teacher voiced such a desire. The form which follows is the defined list presented to the children:

Will you please number these various types of behavior problems in the order of their seriousness. The very worst will be numbered 1, the next worst will be numbered 2, and so on, until you have numbered all 23 of the behavior problems.†

( ) Puppy Love (holding hands, passing notes, one girl always being seen with one boy, or one boy always being seen with one girl)
( ) Stealing (a boy or girl who takes some object which does not belong to the one who is taking it)
( ) Depressed (a boy or girl who is sad, or who seems to lack vitality, or who is dejected or melancholy)
( ) Resentfulness (a boy or girl who feels neglected, or who holds a bitter feeling toward some other person who is present, a boy or girl who shows anger or ill-will, or who is indignant toward some other person)
( ) Shyness (a boy or girl who is bashful or uneasy when some other boy or girl or person is present, or who avoids being with other boys and girls)
( ) Cheating (a boy or girl who cheats or who copies from some other boy or girl during an examination, or when playing a game)
( ) Domineering (a boy or girl who behaves overbearingly, or who demands that his or her orders be carried out regardless of how others, or other children, feel about the matter)
( ) Suspiciousness (a boy or girl who doubts, or mistrusts, or who is always thinking that something is wrong)
( ) Defiance (a boy or girl who is always challenging what someone else has said, or who opposes what someone else wants to do)
( ) Unsociableness (not liking to play or join with other children)
( ) Destroying School Property (writing on the school walls, cutting names or figures on the seats in the classroom)
( ) Heterosexual Activity (playing with boys and girls in the classroom or on the school grounds)
( ) Obscene Notes, Talk (writing notes to some other boy or girl which you would be unwilling to say out loud to the whole class)

* The writer wishes to acknowledge the direction of L. B. Hoisington, University of Oklahoma, in this research.

† Students were not asked to rank "Masturbation."

( ) Cruelty (a boy or girl who seems to hurt some other boy or girl intentionally; a boy or girl who does not seem to care how animals are treated)
( ) Impertinence (a boy or girl who is saucy or insolent, or who is impudent, or who is always talking back in a disrespectful manner)
( ) Untruthfulness (a boy or girl who lies to someone else, or who apparently misrepresents facts)
( ) Fearfulness (a boy or girl who is afraid of something, or who is alarmed, or terrified by someone else)
( ) Truancy (playing hooky from school)
( ) Bullying (a boy or girl who intimidates by physical strength some other person, or who is quarrelsome or swaggering in the presence of someone else)
( ) Overcriticalness (a boy or girl who is always finding more than necessary fault with the work or game being carried on)
( ) Sensitiveness (a boy or girl who is easily hurt by what someone else says about him or her)
( ) Disobedience (a boy or girl who refuses or who does not want to do what he or she is told to do)
( ) Dreaminess (a boy or girl who gazes out the window or aimlessly around the room regardless of the subject being taught)

RESULTS

As stated before, the rankings of the twenty-four varieties of behaviors by 2315 children, parents, apprentice teachers, and teachers, both negro and white, and by 42 child psychologists, constituted the raw material for this study. The average rank assigned to each of the named behavior problems in terms of its significance for the future development of the child is given in Table 1. The coefficients of correlation between the rankings by the various groups are given in Table 2.

An interesting side light of this research appears in the discrepancy between the rankings of those behaviors common to this study and that of Wickman in 1928.[1] A comparison of the rankings obtained by Wickman[2] with those obtained in this study is shown in Table 3. Whether these variances are brought about by a difference between the point of view of clinicians and child psychologists or to a change in the significance attached to these behaviors as influencing personality development, is not certain.

An analysis of the differences between the rankings by the two groups, clinicians and child psychologists, leads to the conclusion that the mental hygienic evaluation of behaviors is tending to move away from the social criterion of adjustment toward an evaluation of behavior in terms of its effect on the developing individual personality. This tendency is indicated by the fact that "depression," "fearfulness," "sensitiveness," "shyness," and "dreaminess" were ranked as more serious by the child psychologists than by the clinicians in Wickman's study.

The most striking fact revealed by these tables is the disparity between the rankings of all other groups and the rankings by the child psychologists. In the case of the teacher groups this lack of agreement indicates that the teachers of child psychology in higher educational institutions are not developing in their students the same attitudes toward children's behaviors as they themselves have or profess to have. The rankings by two groups, negro apprentice teachers and white teachers, yield positive correlations with the rankings by the child psychologists. Although these coefficients are not high, they may have some significance in contrast with the negative correlation of the remaining groups.

[1] Wickman, E. K., *Children's behavior problems and teachers' attitudes*. New York City: The Commonwealth Fund, 1928.
[2] *Ibid.*, p. 127.

TABLE 1

THE AVERAGE RANK IN ORDER OF SERIOUSNESS FOR FUTURE PERSONALITY DEVELOPMENT OF TWENTY-FOUR BEHAVIOR PROBLEMS AS GIVEN BY THE VARIOUS NAMED GROUPS

SUBJECTS	NUMBER	BEHAVIORS AS RANKED																							
		PUPPY LOVE	STEALING	DEPRESSION	RESENTFULNESS	SHYNESS	CHEATING	DOMINEERING	SUSPICIOUSNESS	DEFIANCE	UNSOCIABLENESS	DEST. SCH. PROP.	HETERO. ACTIVITY	OBSCENE NOTES	CRUELTY	IMPERTINENCE	MASTURBATION	UNTRUTHFULNESS	FEARFULNESS	TRUANCY	BULLYING	OVERCRITICALNESS	SENSITIVENESS	DISOBEDIENCE	DREAMINESS
White Children	468	16	1	20	14	22	2	9	13	15	18	4	23	11	5	10	*	3	21	8	7	12	19	6	17
Negro Children	352	17	1	20	12	19	2	11	15	14	16	5	21	13	7	9	*	3	22	6	8	10	23	4	18
White Parents	300	24	1	20	13	22	3	10	16	11	12	15	6	7	4	14	9	2	19	17	8	18	21	5	23
Negro Parents	126	16	1	20	8	21	3	13	17	18	15	6	10	9	5	12	11	2	22	14	7	19	23	4	24
White Ap. Teachers	232	23	1	19	13	22	3	14	16	12	11	9	6	10	4	15	8	2	20	18	7	17	21	5	24
Negro Ap. Teachers	25	21	1	13	9	17	2	8	13	14	10	7	20	18	4	12	18	3	16	15	6	11	14	5	19
White Teachers	514	24	2	13	5	8	3	10	21	12	4	11	22	18	16	7	20	1	19	23	14	17	9	6	15
Negro Teachers	298	22	3	19	5	18	2	13	16	9	14	6	21	12	11	8	23	1	20	15	7	17	10	4	24
Total Subjects	2315	24	1	19	9	21	2	10	17	13	16	6	18	11	5	8	14	3	22	12	7	15	20	4	23
Child Psychols.	42	24	11	1	9	7	16	12	4	13	2	18	23	20	5	19	22	14	3	17	10	15	6	21	8

* Students were not asked to rank "Masturbation."

TABLE 2

COEFFICIENTS OF CORRELATION BY RANK DIFFERENCES BETWEEN THE RANKINGS OF TWENTY-FOUR BEHAVIOR PROBLEMS BY VARIOUS GROUPS

	WHITE CHILDREN	NEGRO CHILDREN	WHITE PARENTS	NEGRO PARENTS	WHITE APPRENTICE TEACHERS	NEGRO APPRENTICE TEACHERS	WHITE TEACHERS	NEGRO TEACHERS	TOTAL SUBJECTS	CHILD PSYCHOLOGISTS
White Children	.	.99	.97	.98	.97	.98	.36	.76	.90	—.148
Negro Children	.99	.	.97	.98	.97	.98	.41	.76	.91	—.075
White Parents	.97	.97	.	.99	.99	.97	.40	.63	.83	—.138
Negro Parents	.98	.98	.99	.	.99	.97	.40	.72	.88	—.333
White Ap. Teachers	.97	.97	.99	.99	.	.97	.41	.65	.85	—.187
Negro Ap. Teachers	.98	.98	.97	.97	.97	.	.73	.84	.84	.271
White Teachers	.36	.41	.40	.40	.41	.73	.	.75	.56	.219
Negro Teachers	.76	.76	.63	.72	.65	.84	.75	.	.86	—.110
Total Subjects	.90	.91	.83	.88	.85	.84	.56	.86	.	—.237
Child Psychols.	—.148	—.075	—.138	—.333	—.187	.271	.219	—.110	—.237	.

Apparently the definition of the behaviors for the children made little difference, for when the rankings of the behavior problems by the children are compared to the rankings of the behavior problems by the parents—who were presented with the behaviors without definitions—the correlation between the two groups is found to be a high positive one. One could conclude from this evidence that attitudes toward these behaviors are formulated on the basis of social environment and have specific word attachments which are not affected by further definition.

TABLE 3

PROBLEM	RANKING BY CLINICIANS IN WICKMAN'S STUDY *	RANKING BY CHILD PSYCHOL. IN THIS STUDY	DIFFERENCE
Unsociableness	1	2	1
Suspiciousness	2	4	2
Depressed	3	1	1
Resentfulness	4	9	5
Fearfulness	5	3	2
Cruelty	6	5	1
Bullying		10	4
Overcriticalness	7	15	8
Sensitiveness	8	6	2
Domineering	9	12	3
Stealing	10	11	4
Shyness	11	7	4
Dreaminess	12	8	4
Truancy	13	17	4
Untruthfulness	14	14	0
Cheating	15	16	1
Heterosexual Activity	16	23	7
Obscene Notes, Talk	17	20	3
Impertinence	18	19	1
Defiance		13	5
Masturbation	19	22	3
Disobedience	20	21	1
Destroying School Property	21	18	3

* The rankings assigned to these behaviors for Wickman's study are not the ones they have in his list of fifty behaviors, but are the ones relative to the behaviors common to the two studies.

Although the negro and white teacher groups correlate positively with all of the other groups except the child psychologists, the correlations are, nevertheless, considerably lower in most instances. These two groups are the only ones which stand out clearly from the other total groups when the rankings by the groups are intercorrelated and the correlations compared, a fact which possibly indicates that training may have a part in bringing about the differences in attitudes for these two teacher groups.

All rankings by negro and white groups correlated positively with one another. The correlations among the parents, children, and apprentice teacher groups, negro and white, were, however, somewhat higher than the correlations between the negro and white teacher groups.

CONCLUSIONS

From the data presented in the preceding pages, these conclusions follow:

1. There is a wide disparity between the rankings of the behavior problems by the child psychologists and all other groups, namely negro and white children, parents, apprentice teachers, and teachers.

2. Rankings by teachers more nearly agree with rankings by parents than with rankings by child psychologists.

3. Rankings by teachers depart from rankings by parents in the direction of rankings by child psychologists.

4. The attitudes of teachers toward children's behaviors are influenced more by home and social environment than by teachings of child psychologists, a fact which gives credence to the conclusion that new methods for teaching the understanding of children's behaviors must be devised if a mental-hygienic point of view be expected of those who supervise children's activities.

5. Child psychologists tend to evaluate behaviors in terms of their effect upon individual personality; teachers and parents in terms of their social consequence.

6. The rankings by children are almost the same as by parents.

7. The rankings by women teachers agree somewhat better with rankings by the child psychologists than do the rankings by men teachers (statistical findings not shown in this survey).

8. Rankings by negro and white parents, children, and teachers reveal no racial difference.

9. Prospective teachers and teachers in attendance at colleges rank the behaviors more nearly as they are ranked by parents than do teachers in service.

THE ATTITUDE OF MEDICAL SCHOOLS TOWARD THE UNDERGRADUATE COURSE IN ABNORMAL PSYCHOLOGY FOR PRE-MEDICAL STUDENTS

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THE writer recently communicated with twenty-five deans of medical schools in an effort to ascertain their attitude toward the advisability of having pre-medical students enrol in a course in abnormal psychology.[1] The question addressed to these medical educators was: "Do you think it advisable for our pre-medical students to enrol in a one-semester course in abnormal psychology? This course would follow a three-hour course in general psychology."

The respondents were asked to check "Advisable" or "Not advisable," and a place was provided for voluntary remarks. The letters of inquiry were sent to deans of the following medical schools: Boston, Cornell, Duke, George Washington, Harvard, Indiana, Iowa State, Johns Hopkins, Kansas, Michigan, Minnesota, Missouri, Northwestern, Ohio State, Oklahoma, Pennsylvania, Rush, St. Louis, Tennessee, Tufts, Tulane, Vanderbilt, Washington (St. Louis), Wisconsin, and Yale.

Replies were received from all of the twenty-five medical schools. Fourteen respondents indicated that they thought instruction in abnormal psychology for pre-medical students was advisable; eleven thought it was not advisable. In only five instances were answers checked without an accompanying remark; one of these was checked "advisable" and four were checked "not advisable." It is not possible to present all of the comments. Only a few of the more pertinent remarks are quoted below; no reference is made to individual names or schools.

. . . . I have made a check under "not advisable." I presume that the question of what is considered normal is determined in psychology as in other fields of investigation by some comparison with the abnormal; consequently, some reference to abnormal psychology is bound to be made in a course in general psychology. Abnormal psychology would seem to be better taught a little later by those trained in psychiatry.

The students need a course in general psychology. They are taught sufficient abnormal psychology in the medical school. I doubt if they are able to profit by a course in abnormal psychology at this stage of their education.

. . . . Of course students who intend to study medicine should have a fundamental course and training in psychology in a modern college. I do not think it matters whether they have abnormal psychology. However, . . . there would be no objection to it. . . . It is not likely that he will get more than he ought to have as a preparation for medicine.

Course in general psychology desirable. Course in abnormal psychology best deferred until second or third year in medical school.

Although we have checked the course in abnormal psychology as being desirable, we feel quite strongly at this school that the requirements for admission to Medical School should be kept to the minimum of required subjects.

I believe that medical schools in general feel that courses in psychology for pre-medical students are quite desirable. However, I think it is the general opinion that the study of abnormal psychology in pre-medical work would be of little advantage to the medical student because abnormal psychology is so closely related to their course in psychiatry, which is a regular requirement in all medical schools.

[1] The writer's interest in this matter grew out of a local situation in which it was thought desirable to discover whether it was advisable to list Abnormal Psychology in the university catalogue as a "suggested" or "recommended" (not required) course for pre-medical students.

I am inclined to favor a course in abnormal psychology provided the course content does not enter the field of psychopathology.

We have not been enthusiastic about abnormal psychology for pre-medics for two reasons. First, one needs a medical background in order to understand these conditions. A pre-medic, therefore, may wonder at what he sees and hears but it is difficult to believe that he really understands much about it. Second, students at this age with slight preparation quite often develop a very morbid interest in the subject which does not seem to serve any very good purpose.

In the main I deplore the tendency to go into abnormal psychology without adequate clinical material and training for the student. I greatly prefer a good foundation in the normal personality study and social psychology.

Abnormal psychology should be given after a student has had some work with other diseases with organic pathology.

I think it would do no harm. My experience is that psychiatrists prefer to give in medical school their own courses in abnormal psychology.

In my opinion much more can be gained from abnormal psychology if it is taken after a student has had some medical preparation for it.

When all of the replies are analyzed, the chief objections to a pre-medical course in abnormal psychology may be summarized as follows:

1. The pre-medical student cannot profit from a course in abnormal psychology; the course should be taken *after* a student has had some medical preparation.

2. The course in abnormal psychology should be taught by a psychiatrist, or by one who has had training in psychiatry.

3. Abnormal psychology is too closely related to psychiatry, now required in all medical schools. In many cases the course in abnormal psychology enters deeply into the field of psychopathology.

4. Abnormal psychology may develop in many pre-medical students a morbid interest in the subject.

5. The course in abnormal psychology puts an additional academic load upon the pre-medical student, who is already weighted down by numerous required subjects.

In view of some of these opinions, the teacher of abnormal psychology is surely given cause for reflection. Is it true that a medical education is required before a course in abnormal psychology can be assimilated? Is it necessary that a teacher of abnormal psychology have professional training in psychiatry? Does the course in abnormal psychology encroach too much on the field of psychiatry?

The majority of the respondents are of the opinion that, without medical training, one is not prepared to interpret abnormal mental phenomena. Regardless of the merits of such a position, may this attitude not arise, in part at least, from the prevalent tendency (both within and without the medical field) to identify abnormal psychology with psychiatry? It is apparent that many medical men are of the opinion that abnormal psychology *is* psychiatry (though, perhaps, a bastard psychiatry). Why, then, they reason, should not abnormal psychology be taught *only* by the psychiatrist? It is to be regretted that psychologists have not been able to convince their fellow scientists who are medically trained that abnormal psychology can be, and is oftentimes taught quite adequately by many "just plain psychologists." Perhaps, however, it is too much to expect at present that psychologists will be very convincing in this regard, inasmuch as psychiatry itself is still looked upon by many medical men as belonging to the "underworld" of their science.

A NOTE ON THE EDITORIAL "WE"

BY FRANCES M. CLARKE
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THE different methods employed by authors to acknowledge sources of data and to introduce a discussion of procedure and findings in experiments previously reported in scientific journals has given rise to the present note. While the comments have general application to all publications, the illustrative material is drawn from a single source because of the variety of practice found in this recent publication (*The startle pattern*, by Carney Landis and William A. Hunt, New York: Farrar and Rinehart, 1939).

The acceptable and commonly used procedure in acknowledging the work of others is the naming of the author or joint authors followed by reference number to the original source. The complete citation—i.e. author, title, journal, etc.—may appear either in a footnote on the page of the text on which the reference appears, or may be found in a compiled list of references appended at the close of the article or book. For example, in *The startle pattern* by Landis and Hunt the plan is followed of listing all references together at the back of the book. This acceptable method is well illustrated in the following examples culled from this source:

1. On page 84 of the text: "It would therefore seem best to restate the original conclusions of Landis, Hunt and Page (**57**). . . ."

2. On page 115 of the text: "This increase in the amount of movement in the spastic limbs is in agreement with the findings of Oppenheim (**66**) and Furnrohr (**2**) but not with those of Landis, Hunt, and Page (**57**)."

Also acceptable are the following examples in which Landis and Hunt report findings of previously published data for which only one of the co-authors shared authorship:

1. On page 130 of the text: "There are results . . . as shown by Beebe-Center and Stevens (**4**) on cats, and Landis and Slight (**58**) on human beings."

2. Also on page 130 of the text: "For instance, Landis and Gullette found that . . . (**52**)."

3. Again on page 130 of the text: "The pulse rate . . . as Landis and Forbes (**51**) have shown. . . ."

On the other hand, the words "we have found," "we have checked," "ours," etc., accompanied by reference number, may be used to report previously published data, in place of the naming the experimenters. Here a confusion, which merits comment, may result if "we" is inconsistently used to mean the authors themselves in one part of the text, and also to mean only one of the authors in collaboration with an independent investigator whose name does not appear in the body of the text in connection with the point being discussed.

For example, from the preface of *The startle pattern* one is led by the statement, "To all . . . we express our appreciation . . ." to assume that "we" as it will be used throughout the text refers to the authors, Landis and Hunt. Yet "we," accompanied by reference number, is also used (specifically in Chapter 5, entitled "Infants and Children," and in Chapter 13, entitled "Secondary Responses") to introduce a description of experiments and results previously reported in print by one of the co-authors in collaboration with another investigator who is not specifically mentioned by the authors in the body of the text. In short, "we found" is used by A and B to refer to work previously published by B and C.

1. One such example can be found on pages 63–68 of the Landis and Hunt publication: "In order to check Strauss's findings, we investigated this type of behavior . . .(36)." Reference to the bibliography included at the back of the book reveals number 36 as "W. A. Hunt, F. M. Clarke, and E. B. Hunt, 'Studies of the startle pattern: IV. Infants,' *Journal of Psychology*, 1936, **2**, 339–52." The phrasing employed in the Landis and Hunt publication to report the experiment is essentially the same as that found in the original journal article; in fact, Table II entitled "Presence of the Moro Reflex" (on page 63 of the text) is identical with Table I of the journal source. There is, however, in the Landis and Hunt publication no specific reference to its previous publication.

2. On page 68 of the text another experiment is reported, introduced in similar fashion: "We performed experiments of this sort on nineteen infants . . .(35)." In the appended list of references number 35 appears as "W. A. Hunt and F. M. Clarke, 'The startle pattern in infants in response to non-auditory stimuli,' *Journal of Genetic Psychology*, 1938, **52**, 443–46."

3. Again on page 71 of the text, the term "we have checked" is used by the authors to cover a detailed report of work which first appeared in the *Journal of Experimental Psychology*, 1937, **21**, 359–62, under the title, "The startle pattern in children and identical twins, by W. A. Hunt and F. M. Clarke."

4. As final illustrations of this practice of referring to an original source by number but not by names of investigators, the following examples might be cited, taken from pages 141 and 142 of *The startle pattern*. Here the word "our" replaces "we" in introducing a discussion of work originally reported by Clarke, Hunt and Hunt in two journal articles. On page 141 of the text there is this statement: "As would be expected, the secondary behavior in infants (9) was more unorganized . . . our work shows that as the infant develops. . . ." And on pages 141–2: "There were also certain overflow effects such as plantar response (10). . . ." Under the numbers 9 and 10 the reader who turns to the appended list of references will find: F. M. Clarke, W. A. Hunt and E. B. Hunt, "Incidental responses in infants following a startle stimulus," *Journal of Genetic Psychology*, 1937, **17**, 398–402; and F. M. Clarke, W. A. Hunt, and E. B. Hunt, "Plantar responses in infants following a startle stimulus," *Journal of Genetic Psychology*, 1937, **50**, 458–461.

In view of the fact that "we" is definitely established in the preface, and also implied on the title page, as referring to Landis and Hunt, exception should be taken to the use of an all-inclusive "we." Such practice leads to confusion, however unintentional. A use of "we" to introduce an account of experiments establishes the data as the work of the joint authors, whereas in reality only one of the present authors contributed to the text the experimental findings from the data previously published in collaboration with a third or fourth unnamed experimenter. This practice would lead those who quote from the work under discussion to attribute the findings in these experiments to Landis and Hunt, instead of to the correct sources. Methodology such as this sets an erroneous example to young researchers.

In conclusion it may be stated that there are, according to editorial practice, only three acceptable uses of the word "we," and the present usage to which exception is taken is not listed among them. It would appear that "we" should be reserved to refer to authors in collaboration with each other, unless a statement is included within the text to indicate that "we" is to have a variety of meaning. Consistent use of the acceptable method of naming joint authors, together with numerical reference to the complete citation in an appended list, would serve even better the interests of scientific report.

REVIEWS

EDITED BY EDNA HEIDBREDER

GENERAL ANTHROPOLOGY. By Franz Boas and others. Boston: D. C. Heath and Co., 1938. Pp. xi+718 (10 plates, 97 figures). $4.00.

No figure in contemporary American anthropology has contributed more to the subject matter or to the stimulation of younger students than Franz Boas. His phenomenal range of learning, his mastery of detail in various related fields, as well as his ability to study problems with reference to a broader theoretical framework, have made him an outstanding scholar and leader. One naturally, therefore, awaited with unusual interest the appearance of the *General anthropology* issued under his editorship, with contributions by him and seven collaborators, former students and close associates. Here, presumably, would be a definitive and well-rounded treatise which would serve as a general introduction to the reader who wished to survey the complex field of anthropology.

Frankly, the book does not come up to expectation. Nor does it achieve in any full measure the purposes that are set forth by Dr. Boas in his introductory section. Part of the difficulty arises from the fact that the fourteen body chapters have been written by eight different people. Dr. Boas states in his preface that "anthropology covers such a wide scope of subjects that it is difficult for one person to be equally conversant with all its aspects." This is the justification for collaboration, but it has led to a sacrifice of that unity which, in so complex and broad a field, is an absolute essential for a general book apparently designed primarily as an introductory text. It was to be expected that the collaborators would vary somewhat in their style of presentation, and in the success with which they organized the materials in the several chapters. Yet greater agreement as to the level of the reader might, it would seem, have been assumed. McGregor's seventy pages on "Human Origins and Early Man" are a wonderfully organized and comprehensive summary, but one which certainly assumes far greater background than the average beginning student or nonspecialist in the subject would possess. On the other hand, Dr. Reichard's chapters are more nearly written down to the level of an introductory student. In general, however, to be read intelligently the chapters do require considerable familiarity with the subjects treated in them. It is as though each author had distilled his vast store of knowledge into the brief span of 75 pages. Elementary or introductory instruction requires over-elaboration rather than over-distillation.

The book, however, should be judged in somewhat broader terms than these. In his introduction Dr. Boas writes that "the science of anthropology deals with the history of human society," and he stresses the laws of historic development and the reconstruction of human history, the determination of types of historical phenomena and their sequences, and the dynamics of cultural change as "the three great problems of anthropology." Here, obviously, there is need for systematic treatment, both in presentation of data and in the making of interpretations, generalizations, or conclusions. The task that is outlined clearly demands unity and integration of material. In this reviewer's opinion the chapters of the book, taken together, do not provide a basis for a better under-

standing of the three great problems, nor for developing a sensitive understanding of the manner in which the aspects of any culture are integrated. The treatment, by and large, is descriptive and static, not descriptive and dynamic. Although facts are present by the thousands, they exist without a general integrating framework. One suspects that a framework was clearly in the writers' minds, and accepted by them; but their reader does not have the help of the framework in drawing the fourteen chapters together.

Aspects of the problems of anthropology that would be of most interest to readers of this JOURNAL are either deliberately omitted or receive only casual treatment. Thus the relation of personality to culture, the educative process, acculturation, the dynamics of change, the role of the individual, and similar problems falling close to the field of social psychology are excluded, or are submerged in the more formal descriptive matter. The too brief introduction of these in "Methods of Research," with which the book concludes, is not sufficient to balance the plethora of earlier detail.

The book needs a glossary, not only to include some technical terms that are used, but tribal names. There are a hundred or more primitive groups mentioned, often without any clue to geographical location.

Perhaps it was not possible to organize the book other than by the conventional chapters as they appear at present, but one wonders if a sense of cultures as living things could not have been achieved through some other approach. Minute descriptions of variations in cultural practice do serve as an antidote to glib generalization concerning man and his development, and may help to break down tendencies toward ethnocentrism; but when carried to an extreme, the result is like trying to give a man who has never seen a tree a comprehension of "trees" by describing the minute variations in each individual leaf. In a general anthropology what one needs is an integrating factor, whether it be in terms of geographical distribution, or methodological concepts, or some other conceptual factor. It is this factor that is lacking in the present book.

The individual chapter contributions are as follows: Introduction (Boas); Geological and Biological Premises (N. C. Nelson); Human Origins and Early Man (James H. McGregor); Race (Boas); Language (Boas); Prehistoric Archaeology (Nelson); Invention (Boas); Subsistence (Robert H. Lowie); Economic Organization of Primitive Peoples (Ruth Bunzel); Social Life (Gladys Reichard); Government (Julius Lips); Art (Bunzel); Literature, Music and Dance (Boas); Mythology and Folklore (Boas); Religion (Benedict); Methods of Research (Boas).

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EMOTIONS. By Frederick H. Lund. New York: The Ronald Press, 1939. Pp. xiii+305.

This book is based on the report of recent researches on emotion made by Dr. Lund to the Committee on the Relation of Emotion to the Educative Process. In his preface Dr. Prescott, Chairman of the Committee, writes of it: "This volume brings together and organizes a great mass of research findings which heretofore could be uncovered only by a laborious examination of the periodical literature and of numerous other books." On examination, however, the 305 pages of this book prove to be not a comprehensive survey suited to the needs of the research worker, but rather more appropriately a text for the

advanced undergraduate student interested in this particular topic. It is with such ultimate use in mind that the present evaluation is attempted.

The space devoted to the different aspects of emotional behavior is in accordance with Dr. Lund's thesis that, while the affective, facial, and skeletal components must be recognized as important indicators of emotion, and while "the very complexity of the processes . . . makes it impossible to single out any one segment and regard this as *the* emotion, . . . it is in the changes of the autonomic system that we have the most conclusive evidence of emotional stimulation and arousal" (p. 287). Of the eight chapters covering the experimental literature, five are concerned with autonomic and visceral changes.

An introductory chapter devotes 31 pages to the problems involved in the proper identification of the emotions. Certain studies on the recognition of emotional states from facial expression are reviewed here. The next five chapters deal with physiological changes in emotion (neuro-glandular bases of emotional reactions, cardio-vascular and respiratory changes, gastro-intestinal and sexual changes, and metabolic and skin changes). These chapters give, on the whole, a clear, detailed picture of the current knowledge of these fields. The nature of the physiological changes themselves and the methods of measurement are especially well presented. Somewhat less successful, because the evidence is so widely scattered through these chapters, is the discussion of the relation of such physiological changes to specific stimulating conditions on the one hand, and to affective states on the other. Noteworthy is the attempt to bring together what little is known of the bodily processes accompanying the pleasant emotions.

The treatment of "The Development and Control of Emotions" in a seventh chapter is far less comprehensive. This chapter is concerned with somatic emotional reactions, their development, and the factors involved in the control and restraint of the emotional experience. The discussion of early developmental aspects is largely limited to such topics as conditioning of emotional responses, criticism of Watson's pioneer formulations in the light of the more recent data of Sherman, Bridges, and others, and evaluation of the theory that specific reactions develop gradually by a process of individuation. There is only the briefest reference to the findings of numerous recent monographs in which age changes have been studied through time-sample and questionnaire techniques. No mention is made of such specific emotional responses as the Moro reflex and the startle pattern. The virtually complete omission of psychoanalytic studies of emotional development is justified with the reason that they "are entirely too subjective and descriptive to be fitted into an experimental account" (p. 284).

More specific objection may be made to Dr. Lund's conception that "maturation provides for growth and differentiation of the principal patterns of response, while learning provides for conditioning or for attachment of these response modes to particular objects and situations" (p. 242). This lack of recognition of the important influences of cultural factors on manner of emotional expression is particularly surprising in view of such a remark as the following, in a much earlier chapter: "That certain facial patterns become fixed and uniform through common training is fairly obvious" (p. 29).

"Emotion and Motivation," the subject of the eighth chapter, is interesting for its emphasis on the "close relationship between the conditions identified with emotion and those identified with motivation" (p. 276). Methods of measuring motives in animals and men, and the effects of various motives on performance receive conventional treatment.

In addition to the general criticism that the book is not well-rounded, and

that the treatment of the different topics is uneven in merit, a number of specific criticisms may be made. Dr. Lund seems needlessly to have confused the problem of definition by repeated attacks on older mentalistic formulations. Today, when the scientific status of psychology is taken for granted, any vigorous repudiation of either vitalistic interpretations or of a dualistic approach to psychological phenomena seems unnecessary. It is this concern with an objective formulation that may have caused Dr. Lund to slight the treatment of the affective component of emotional behavior.

In a number of instances, the book shows evidence of insufficient care in revision. Reference is made on page 37, for example, to non-existent dot-and-dash lines in a diagram. Again, there is a reference on page 230 to "the content of some of the personality inventories discussed in the previous chapter." No such discussion appears in the book. Further indication of careless editing is apparent in a rather general repetitiousness and wordiness, and in such an assertion as the following from page 74: "The viscera are relatively insensitive structures as is apparent in the absence of pain or discomfort in an anaesthetized patient whose alimentary tract is subjected to severe treatment such as cutting, burning, or tearing."

Faults such as these are to be regretted because in many ways the book should prove valuable. There is a real need for comprehensive texts on specialized topics suited to the advanced student in psychology. With this book, especially in those chapters which deal with the physiological aspects of emotional behavior, Dr. Lund has made a useful contribution toward filling such a need in the field of emotion.

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PSYCHOLOGICAL FOUNDATIONS OF PERSONALITY. By L. P. Thorpe. New York: McGraw-Hill, 1938. Pp. xvi+602. $3.50.

It is difficult today to form a lasting conception of personality. No sooner has the student of human nature decided that he knows what personality is than he comes across a brand-new conception which forces him to abandon his former ideas, or at least seriously to modify them. While such modification of ideas is the essence of intellectual growth, it has a disturbing effect upon the young student who is trying to form a background of knowledge about human nature in order that he may advance to further knowledge.

Fortunately for student and teacher alike, there has appeared a work by Professor Thorpe, entitled *Psychological foundations of personality*, a book which, as far as personality is concerned, does away to a large extent with this danger of a constantly disappearing background. Professor Thorpe is not hesitant in giving us a cross-section of the voluminous literature on personality, thus revealing to the reader the nature of personality from the different psychological viewpoints. To achieve his aim, he dips freely into child psychology, the psychology of adolescence, educational psychology, abnormal psychology, and most of the other branches.

Beginning with a survey of the different conceptions of personality—such as personality as the social self, or personality as an aggregate of components, commonly known as traits—Professor Thorpe goes on to show the biological foundation of personality. This, he holds, is the physical organism with all its structures and functions. Beyond this nothing is genetically inherited. The physical organism provides the soil in which the roots of personality take hold.

Personality itself, however, is largely a matter of the experiences accumulated by the budding individual as he comes more and more into contact with the social environment around him.

This trend of thought is carried further in a discussion of the educability of personality. Here Professor Thorpe suggests that even intelligence, as one of the elements of personality, can be significantly changed by social surroundings. While investigators of the nature of intelligence are not agreed as to the extent of this change, many of them contend that the change can be effected, and that favorable opportunity thus has a decided influence on the improvement of an individual's intelligence.

The establishment of the fact that personality, in terms of the intelligence factor, finds its beginning in the genes, but that this beginning is immediately enhanced by the ever-developing environment of the genes, leads Professor Thorpe to examine the forces which cause the individual to develop from a number of potentialities into a complex self. The different motives that underlie man's behavior are scrutinized, and placed in turn alongside the affective arbiters of man. In dealing with the emotions, Thorpe is concerned as to whether they are direct stimulators of action, or merely accompaniments of action. He decides in favor of the second conception.

All this leads Professor Thorpe to an examination of the constituents or traits of personality. The traits are the bricks used to build the house of personality; the foundations are the genes, the emotions the mortar. The motives are the builders themselves. Once these four factors of personality are brought together, there is little left to do but wait to see what the finished house of personality will look like. According to Professor Thorpe, however, the house of personality is never finished. Integration goes on continuously, transforming the house into a beautiful home.

Even if there were an end to the integration of personality, the individual could never rest assured that his well-rounded personality would remain unchanged. Inasmuch as the factors leading to disintegration are always around him, he must remain constantly on guard.

The study of the psychological foundations of personality is not complete, however, unless the individual is told how to go about measuring the components of personality, and so Professor Thorpe devotes a sizable portion of his book to psychological measurements of personality. As an educator Professor Thorpe is especially interested in the possibility of molding personality. All through the book the belief in the educability of personality shines out hopefully.

Professor Thorpe's book is one that may be whole-heartedly recommended. It is especially valuable in that it presents the case of personality in an objective manner without trying to impose the author's point of view upon the reader. The latter, given the facts, is allowed to decide for himself. In a book directed to teachers and students this attitude is a commendable one, and will be received with a great deal of appreciation.

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